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SOLIDS, LIQUIDS, AND GASES.

By W. Mattieu Williams.

PART I.

THE growth of accurate knowledge is continually narrowing, and often obliterating the broad lines of distinction that have been drawn between different classes of things. I well remember when our best naturalists regarded their "species" of plants and animals as fundamental and inviolable institutions, separated by well-defined boundaries that could not be crossed. Darwin has upset all this, and now we cannot even draw a clear, sharp line between the animal and vegetable kingdoms. The chemist is even crossing the boundary between these and the mineral kingdom, by refuting the once positive dictum that organic substances (*i.e.*, the compounds ordinarily formed in the course of vegetable or animal growth) could not be produced directly from dead matter by any chemical device. Many of such organic compounds have now been made in the laboratory from mineral materials.

We all know broadly what are the differences between solids, liquids, and gases, and, until lately, they have been very positively described as the three distinct states or modes of existence of matter. Mr. Crookes suggests a fourth. I will not discuss this at present, but merely consider the three old-established claimants to distinctive existence.

A solid is usually defined as a body made up of particles which hold together rigidly or immovably, in contradistinction to a fluid, of which the particles move freely over each other. "Fluid" is the general term including both gases and liquids, both being alike as regards the mobility of their particles. At present, let us confine our attention to liquids and solids.

The theoretical or perfect fluid which is imagined by the mathematician as the basis of certain abstract reasonings has no actual existence. He assumes (and the assumption is legitimate and desirable, provided its imaginary character is always remembered) that the supposed particles move upon each other with perfect freedom, without any friction or other impediment; but, as a matter of fact, all liquids exert some amount of resistance to their own flowing; they are more or less *viscous*, have more or less of that sluggishness in their obedience to the law of finding their own level which we see so plainly displayed by treacle or castor oil.

This viscosity, added to the friction of the liquid against the solid on which it rests, or in which it is enclosed, may become, even in the case of water, a formidable obstacle to its flow. Thus, if we make a hole in the side of a tank at a depth of 16 feet below the surface, the water will spout from that hole at the rate of 32 feet per second, but if we connect with this hole a long horizontal pipe of the same internal diameter as the hole, and then observe the flow from the outlet of the pipe, we shall find its velocity measurably diminished, and we shall be greatly deceived if we make arrangements for carrying swift-flowing water thus to any great distances.

Three or four years ago an attempt was made to supersede the water-carts of London by laying down on each side of the road a horizontal pipe, perforated with a row of holes opening towards the horse-way. The water was to be turned on, and from these holes it was to jet out to the middle of the road from each side, and thus water it all. I watched the experiment made near the Bank of England. Instead of spouting across the road from all these holes, as it would have done from any *one* of them, it merely dribbled, the reason being that in order to supply them all, the water must run through the whole of the long pipe with considerable velocity, and the viscosity and friction to be overcome in doing this nearly exhausted the whole force of water-head pressure. Many other similar blunders have been made by those who have sought to convey water power to a distance by means of pipes of such diameter as should demand a rapid flow through a long pipe.

The resistance which water offers to the stroke of the swimmer or the pull of the rower is partly due to its viscosity, and partly to the uplifting or displacement of some of the water. If it were perfectly fluid, our movements within it, and those of fishes, &c., would be curiously different, and, in fact, the whole face of this globe would be strangely altered in many respects. I will not now follow up this idea, but leave it as a suggestion for the reader to work out for himself, by considering what would remain undone upon the earth if water flowed perfectly, without any internal resistance.

The degrees of approach to perfect fluidity vary greatly with different liquids.

Is there any such a thing as an absolute solid, or a body that has no degree of fluidity, the particles or parts of which will admit of no change of their relative positions, no movement upon each other without fracture of the mass? This would constitute perfect *rigidity*, or the opposite to *fluidity*.

Take a piece of copper or soft iron-wire, about one-eighth of an inch in diameter, or thereabouts, and bend it backwards and forwards a few times as rapidly as possible, but without breaking it; then, without loss of time, feel the portion that has been bent. It is hot—painfully so—if the experiment is smartly made. How may this be explained?

It is evident that in the act of bending there must have been a displacement of the relative positions of the particles of the metal, and the force demanded for the bending indicated their resistance to this movement upon each other; or, in other words, that there was friction between them, or something equivalent to such internal friction, and thus the mechanical force exerted in the bending was converted into heat-force.

Here, then, was fluidity, according to the above definition; not perfect fluidity, but fluidity attended with

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resistance to flow, or what we have agreed to call viscosity. But water also offers such resistance to flow or viscosity, therefore the difference between iron or copper-wire and liquid water as regards their fluidity is only a difference of degree, and not of kind; the demarcation between solids and liquids is not a broad, clearly-defined line, but a band of blending shade, the depths of tint representing varying degrees of viscosity.

THE PHILOSOPHY OF ANIMAL COLOURS.

By Dr. Andrew Wilson, F.R.S.E.

PART II.

BUT, descending to still lower grades of life, we may discover examples of this "mimicry," not only of surroundings, but also of lifeless or inorganic objects, and of, it may be, plant structures as well, on the part of animals. The so-called "stick insects," or "walking twigs," as they are often called—the *Phasmidæ* of the naturalist—present us with the most perfect reproductions of bits of dried twigs. A figure of one of these insects is before me as I write. It is represented climbing on the delicate branch of a shrub, and but for the expectation of what one is looking for, there would be considerable difficulty in determining which is insect and which plant. The bodies of these "twig insects"—which belong, by the way, to the *Orthoptera*, or that order which harbours the familiar crickets and grasshoppers—are represented by mere lines. The wings have disappeared, and it has been remarked that in their gait these insects exhibit a peculiar habit of using their legs in a singularly awkward fashion, and thus apparently aid the illusion of the spectator that he is regarding a dried twig, moved erratically by the wind.

More extraordinary still are the "Leaf insects;" near allies, indeed, of the walking-sticks." Here "mimicry" of the plant proceeds so far as to fully justify the eminent naturalist's remarks, that it is strange to find the animal assuming a mimetic disguise and apeing the actor's art. The wings in the "leaf insects" exactly imitate leaves. The venation, or arrangement of the veins in the leaf, is clearly seen, and in one form (*Phyllium*) even the chest and legs of the animal assume leaf-like characters. When such an insect rests amid foliage, the value of such a close resemblance to its plant surroundings as a means of protection can be readily understood. In some "leaf insects"—all of which are tropical species—the wings resemble leaves that are dried and withered. In others, the minute fungi that attack leaves are imitated. Mr. A. R. Wallace tells us that one of the "walking-sticks" obtained by him in Borneo, "was covered over with foliaceous excrescences of a clear olive green colour, so as exactly to resemble a stick grown over by a creeping moss or Jungermannia. The Dyak who brought it me assured me it was grown over with moss, though alive, and it was only after a most minute examination that I could convince myself it was not so."

Lastly, there may be noticed in connection with these curious traits of animal life, the fact that certain animals, themselves harmless and inoffensive, may assume the exact appearance of offensive neighbours. In this respect, certain butterflies are facile principe. Certain South American butterflies, known collectively under their family name of *Heliconidæ*, exhibit a brilliant colouration, but likewise possess a very strong odour; and, it may be presumed from the sequel, a highly disagreeable taste as well. They are highly conspicuous insects, and the under sides of their wings are as brilliantly coloured as the upper surfaces; so that, even in repose, and when resting with the wings apposed over the back, they are readily enough seen. Their colours are prominent, not to say gaudy. Yellows, reds, and whites, commingle with blacks, blues, and other tints in a striking fashion. They are, further, by no means rapid flyers, and, putting the foregoing circumstances of their gaudy colour and their slow movements together, no group of animals would seem more liable to the attacks of bird-enemies than these Helicon butterflies. Yet the reverse is the case. So far from being decimated, their race flourishes apace, and this result is clearly due to the strong odour and nauseous taste they possess. The mere touch of a Helicon is in itself a pungent matter, which reminds one of nothing so much as the persistence of the musk-rat's secretion, or the still more awful effluvium of the American skunk. Their neighbour butterflies may fall victims by the score to the rapacity of their feathered enemies, but the Helicons are spared from even the semblance of attack.

So far there seems nothing unusual or striking in a group of butterfles being protected, through strong odour and worse taste, from their natural enemies, the birds. But now comes the most curious phase of this history. Another and distinct family of butterflies, known as the *Leptalidæ*, allied to the common white cabbage butterfly, and removed from the Helicons, also possesses representatives in South America. There are no points of agreement between the Leptalides and the Helicons, save, indeed, that both are butterflies. Furthermore, the Leptalides are entirely destitute of the nauseous odour and of the strong taste of the Helicons, and in respect of their more agreeable presence, should become a prominent article—as do other butterflies—in the bill of fare of the birds. Yet, strangely enough, the Leptalides escape persecution; and the reason is not far to seek or difficult to find. When they are carefully examined, certain species of the Leptalides are seen to be exact *facsimiles*, in colour and appearance, of the stinking Helicons! Naturalists at first classed both as Helicons, until a closer examination showed the difference between these butterflies, and likewise proved that the Leptalides had thus "mimicked" in the plainest possible manner the colours of their strong-smelling neighbours. Nor are the colours alone imitated. The very shape of the Helicon's wings is reproduced in those of the Leptalides, and the "feelers" likewise mimic those of the former group. Again, special forms of Leptalides "mimic" special forms of Helicons. The flight has become of similar character in both species, and the habits have been also slavishly copied.

Such instances as these certainly present "food for thought" to the reflective mind. It is the business of philosophy to account for facts by placing the facts in scientific juxtaposition—philosophy, in this light, is the thread upon which the pearls of knowledge are strung. What, then, it may be asked, is the philosophy which can explain the curious resemblances seen in the animal world, ranging from, say, a mere likeness in tint to the surroundings (as in the flounder or woodcock), through more intensified likenesses, to the exact "mimicry" and to the slavish copy of colour and form, as in the butterflies?

A first and highly important feature in the consideration of the case is found in the fact that there is a gradation in the degree of "mimicry." From the mere sand or ground tinting of the flounder to the exact colouring of the butterflies is, of course, a wide step, but it is one which is bridged over by intermediate examples and stages. Then, secondly, we discover a purpose or use in the disguises: that purpose, apart from any considerations of its origin, being the protection of the animal from its enemies, and the con-

sequent good and increase of its race. Thirdly, it appears possible to account for these curious transformations and disguises, by finding an *initial* step. It is the old story of *le premier pas qui coûte*, applied to natural history research; and this first step is found in the solid axiom, that every living species is liable to *variation* and change. Next succeeds the consideration that such varieties as are produced have to "struggle for existence." Suppose a number of white varieties produced in a cold, snowy region, along with varieties of more conspicuous colours. It is evident that, whilst the white varieties would escape from their enemies, the darker-coloured individuals would succumb. Thus the white race comes to the front, and holds its own, and its perpetuation and increase becomes a matter of surety. Summing up the argument, we find that two factors are at work in bringing about these wonderful colour likenesses in the animal world. The one is variation, producing the colour-varieties; the other is the circumstances of life, which weed out the weak and give the battle to the strong, which latter are those whose colours best suit their surroundings. This is the philosophy which natural history to-day lays down for our acceptance. Nay more, it is a philosophy which explains far more important facts of life than mere mimicry. It is "evolution and development" reduced to their plainest and fundamental terms—in a word, Darwinism in a nutshell, as illustrated by the variation and change that all life knows, and by the warring of that life bringing the best of its units to the front of the battle.

BRAIN TROUBLES.

Impaired Memory.

THE first mind trouble we propose to consider is the apparent temporary impairment of the memory. The gradual progressive decay of the power of memory with advancing years is, of course, a change which all may expect who attain great length of years; though, as Cicero long since pointed out, and as has been repeatedly exemplified by modern instances, the change is to some degree under control, and those matters in which an aged person takes special interest may be well remembered, when others about which he cares little are easily forgotten. "I never heard," says Cicero, "of any old man that had forgotten where he had hid his treasure; things which they regard, old men remember—the securities they have out, and who are indebted to them, as well as to whom they are indebted." And so forth. The points to be noticed here are, first, that memory is seen to be in large degree a question of attention as well as of retention; and, secondly, that decay of memory implies a change in the mind analogous to that which makes the old incapable of great bodily efforts. So that when the memory of a person who is not old becomes impaired, we may infer that unless there is actual disease, the symptom indicates overwork of the mind, precisely as bodily weariness indicates that the body has been overwrought. We may, perhaps, be led to inquire here whether a distinction should be drawn between loss of memory, as shown by a weakening of the power of committing to mind new matter (of whatever kind) which we may wish to remember, and the passing away from the mind of matter which had been already committed to it, and retained so long and so recently that its being forgotten can be explained only as due to some marked and recent change in the state of the mind. Suppose, for instance, that after carefully noting a number of facts, which under ordinary conditions we should remember thenceforward for weeks, we find that they have left no sufficient impression on the mind; here we obviously have evidence that the power of attention on which, in the first instance, memory depends, is for the time being enfeebled. Can we, however, infer that it is weakened in the same way and in the same degree as we should judge it to be if we found that numbers, dates, names, or words which we had had occasion to refer to daily for years, were suddenly clean forgotten? Making use, as we conveniently may (though we must not place too much stress on the method), of the analogy between bodily and mental relations, we may compare a change of the former kind to a diminution of the power of acquiring some new feat; a change of the latter kind, to the sudden loss of a feat already acquired and long practised. It can hardly be doubted that an athlete who should find himself unable to perform some new gymnastic trick, which he had supposed well within his powers, would not be so much struck by the circumstance, as he would be if he should suddenly find himself unable to achieve a feat in which he had hitherto found no difficulty.

Let us inquire, however, whether known cases of loss of memory of either kind afford any means of answering the question which has thus arisen. Of course, those cases in which the trouble has been only temporary, though far more numerous than those in which loss of memory has been symptomatic of actual disease, stand far less chance of being kept on record, so that we may have to consider cases of the latter kind to discover the relative importance of the two forms in which loss of memory may be noticed. The reader must not judge from cases thus cited that either class of symptoms is necessarily, or even probably, indicative of serious brain mischief.

We will begin, however, with a case in which the trouble was only temporary, and, moreover, its cause obviously indicated.

Sir Henry Holland gives the following suggestive account of a transient loss of memory due to fatigue. It will be seen that the failure of memory belongs to the second class above referred to, that, viz., in which what had been long and well known is suddenly forgotten. "I descended," he says, "on the same day, two very deep mines in the Harz Mountains, remaining some hours underground in each. While in the second mine, and exhausted both from fatigue and inanition, I felt the utter impossibility of talking longer with the German Inspector who accompanied me. Every German word and phrase deserted my recollection, and it was not until I had taken food and wine, and been some time at rest, that I regained them." This case would seem to show that transient loss of memory, even of this kind, need not be regarded as necessarily an alarming symptom.

The following case points in the same direction even more decisively. "A gentleman," says Dr. Winslow, "well known for his intense passion for field sports (living, it may be said, upon the saddle during the greater part of the year), frequently complained of transient attacks of loss of memory after a hard day's run with the hounds. His remedy for this affection was half-a-pint to a pint of port wine *at a draught!* The effect of this heroic dose of vinous stimulant upon the depressed energy of the brain was evidenced by the memory immediately recovering its vigorous activity." It would, however, be unwise to infer that this sportsman, in thus prescribing for these attacks, showed himself the "physician" of the proverb relating to fools and physicians. The remedy was a dangerous one. His was specially a case where prevention was better than cure. The transient attacks of loss of memory showed that the hard day's run with the hounds overtaxed his strength. He would have done wisely to have limited his exertions

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in the field (not giving up hunting, but restraining his zeal on those occasions when the day's run promised to be harder than usual).

The effect of wine used regularly, not in pint draughts, is in many cases undoubtedly good where the memory is apt to fail. We have an illustration of this in the following case, belonging to the first of the two classes above considered. A gentleman whose mental and physical powers had been severely taxed, lost all power of recollecting recent events. "Whilst engaged in active conversation, he was able, by a strong effort of the will, to retain possession of the ideas suggested by others to his mind; but if there were the slightest interruption, even to the extent of a minute, in the conversation, he lost all recollection of what he had been previously saying. This gentleman had been living for some weeks below par, with the view of enabling him to perform an amount of urgent mental work, requiring for its execution the lengthened concentration of a clear and vigorous intellect. He had been in the habit of drinking a fair portion of wine, but had unwisely abandoned the use of stimulants, fancying that by so doing he would be better fitted for clear-headed mental occupation." Under Dr. Forbes Winslow's advice, the patient "lived generously, took iron tonics, quinine, and valerianate of zinc, and resumed his daily quantity of wine. This treatment eventually restored his memory to a state of health." Dr. Forbes Winslow adds that he has known other instances of temporary loss of memory cured within a short time by the free use of tonics and stimulants. "In these cases," he says, "the brain is generally in a starved and impoverished condition, arising from a deficient supply of blood; it is in a state of enervation and inanition." On the other hand, the excessive use of stimulants produces unmistakably mischievous effects. Temporary attacks of loss of memory have been caused by intemperance. "By an old Spanish law," Dr. Winslow mentions, "no person was admitted into the witness-box to give evidence in a disputed case who was proved to indulge in habits of intemperance, as an excessive use of stimulants was considered to weaken and destroy the memory."

(*To be continued.*)

INTELLIGENCE IN ANIMALS.

PART II.

THE next case cited also relates to the apparent exercise of reasoning faculties by rats, and is interesting, because probably their action was guided by the sense of hearing, rather than by that of smell. "Some years ago," says the narrator, "a plumber told me that he had, on several occasions, been called in to examine into the cause of leakage of water-pipes under the flooring of houses, and had found that the rats had gnawed a hole in the leaden pipe to obtain water, and that great numbers of them had made it a common drinking-place, as evidenced by the quantity of dung lying about. The plumber brought me a piece of leaden pipe, about three quarters of an inch in diameter, and one-eighth of an inch in thickness, penetrated in two places, taken by himself from a house on Haverstock-hill. There are the marks of the incisors on the lead as clear as an engraving; and a few hairs and two or three of the rat's whiskers have been pinched into the metal in the act of gnawing it. This crucial proof of brute intelligence—for a rat will not drink foul water—interested me so much that I ventured to send an account of it to Dr. Charles Darwin, asking his opinion on the means by which the rats ascertained the presence of water in the pipe. To this he replied: 'I cannot doubt about animals reasoning in a practical fashion. The case of the rats is very curious. Do they not hear the water trickling?'" This explanation would go far, it would seem, to do away with the idea that the rats in this case had reasoned, seeing that if they recognised the presence of water by the sense of hearing, their action in biting their way through to what they wanted would correspond precisely with what we have been taught (erroneously, in all probability, but that is a detail) to regard as instinctive. The narrator, however, did not read Dr. Darwin's reply in this sense. "It may be conceded," he says, "that this explanation is the most probable, and if it be the true one, we have an example of an animal using his senses to obtain the data for a process of reasoning leading to conclusions about which he is so certain that he will go to the trouble of cutting through a considerable thickness of lead. Obviously man could do *no more* under the same conditions." If the rats had shown in their boring operations some special aptitude for securing most conveniently, with the least possible overflow, the water they required, this would be a just inference. But as we know no more than that, having found, probably by the sense of hearing, that water was present in the pipe, they bored their way through to reach it, we have in reality no more proof of reasoning power than is afforded by the familiar action of mice in biting their way through the wooden or card casings of boxes of edibles they like, of whose presence within such boxes the sense of smell has convinced them.

This objection is well put by Mr. Henslow in a letter discussing this particular case, and Dr. Darwin's comments thereon, only, as it seems to us, he carries the objection rather farther than it will fairly go, extending it to cases to which we think it can hardly be applied. "It has always seemed to me," he says, "that brute reasoning is always *practical*, but never *abstract*" (but he tries to show that there is very little reasoning at all in the matter). "They do wonderful things, suggested by the objective fact before them; but, I think, never go beyond it. Thus, a dog left in a room alone, rang the bell to fetch the servant. Had not the dog been taught to ring the bell (which, on inquiry, proved to have been the case), it would have been abstract reasoning; but it was only practical. The Arctic fox—too wary to be shot, like the first, who took a bait tied to a string, which was attached to the trigger of a gun—would dive under the snow and so pull the bait down below the line of fire. This is purely practical reasoning; but had the fox pulled the string first out of the line of fire, *in order* to discharge the gun, and *then* to get the bait, that would have been abstract reasoning, which he could not attain to." This, however, is assuming more than can be proved; the fox in the case referred to did not act in the way which would have implied abstract reasoning; we do not know that no fox has ever done so, still less that, failing a simpler way of attaining his object, no fox could so reason. Albeit, we believe there are very few cases in which a line of reason involving so many steps as that suggested by Mr. Henslow has been followed by an animal. Mr. Henslow makes a good point in noting how like the practical reasoning of animals is the reasoning of young folk. "A boy the other day," he says, "found the straps of his skates frozen. The fact only suggested *cutting* them. Not one of his schoolfellows reflected upon the abstract fact that the ice would melt if he sat upon his foot a few minutes. Hence brutes and boys are exactly alike in that nothing occurs to either beyond what the immediate fact before them may suggest. The one kind I call purely *practical* reasoning, which both have; the

other abstract, which brutes *never* acquire; but the boy *will* as his intelligence develops."

Certainly the next case cited in the correspondence suggests practical rather than abstract reasoning. "In Central Park, one very hot day, my attention," writes Mr. James J. Furniss, of New York, "was drawn to the conduct of an elephant which had been placed in an inclosure in the open air. On the ground was a large heap of newly-mown grass, which the sagacious animal was taking up by the trunkfull, and laying carefully upon his sun-heated back. He continued the operation until his back was *completely thatched*, when he remained quiet, apparently enjoying the result of his ingenuity. It seems to me that *instinct* should have prompted the elephant to eat the grass, and that it was reason which caused him to use it for the purpose of diminishing the effect of the sun's rays." Undoubtedly, had hunger been the prevailing sensation at the time, instinct would have caused the elephant to eat the grass. But, as he was probably much troubled by the heat, it was not more wonderful that he should throw grass on to his back, than it would have been if, had there been any shadow, he should have withdrawn under it. Doubtless, however, the true explanation is that the elephant reasoned in a practical way. The effect of the grass as a protection from the heat was obvious to his senses, so he continued to add more and more grass to his covering until he felt comfortable. If the use of the grass for food occurred to him at all, it would have appeared obvious enough that even if all the grass were used for shelter, it would be none the less suitable for food when hunger began to be troublesome.

ARE WOMEN INFERIOR TO MEN?

M. DELAUNAY next proceeds to compare the brain capacity of men and women. Huschke estimates the mean capacity of the cranium for Europeans to be 1,446 cubic centimètres (about 88¼ cubic inches) for men, and 1,226 (about 74⅘ cubic inches) for women, or the masculine brain exceeds the feminine, on the average, nearly 18 per cent. in capacity. However, before we too hastily assume that this implies inferiority, we may as well consider the relative dimensions of men and women in other respects. We have to take into account rather the relative than the actual dimensions of the brain. Now, the average height of men in European countries exceeds that of women by about one-seventeenth part, that is, men are taller than women in about the ratio of 18 to 17. Men, therefore, exceed women in bulk in about the ratio of 18 times 18 times 18 to 17 times 17 times 17 (for the volumes of bodies vary, not as their linear dimensions, but as the cubes of these dimensions), or rather more than the ratio of 118 to 100. Hence, so far as Huschke's estimate can be trusted, the cerebral capacity of women is relatively greater than that of men.

We learn from Broca, though one would like to be assured that the statistical evidence is trustworthy, that the brain of man is heavier than that of women as 111 to 100. This, like Huschke's evidence, but in even greater degree, would be unfavourable to M. Delaunay's position. But Broca also notes that the female cranium is longer and less high than the male. The broader-headed among men will probably regard this as evidence of inferiority, while the dolichocephalic, or long-headed, will regard it as proof positive that at any rate women have equal, or probably superior, brain fitness, so far as shape is concerned.

"The graphic curves of feminine brains of various races," says M. le Bon, "shows that even in the most intelligent societies, as the Parisians of to-day, there is a notable proportion of the female population whose brains approach more to the volume of those of certain gorillas than the least-developed crania of the masculine sex." This sounds unflattering, but in reality it means very little. For the worst-developed crania which are here compared include, both among male and female specimens, abnormal cases, from which it is not easy to infer the true relations of brains of that class. Moreover, the men of worst-developed brains are more apt to get eliminated from society, so that the very worst specimens of masculine crania being removed, the comparison between the bad male and the bad female heads is affected unfavourably for the latter.

The frontal lobes, "the seat of the highest intellectual faculties, are less developed in woman than in man, while, on the other hand, the occipital lobes, which especially preside over the life of sentiment, are more voluminous in the woman than in the man." In its totality, according to Professor Wagner, "the brain of the woman is always in a state more or less embryonic." This must, of necessity, relate to averages, not to individual brains. If the brains of those women who have been distinguished for genius were compared with the brains of ordinary men, there would not be found any evidence of a state more or less embryonic. This, of course, would by no means dispose of the argument, but it indicates a circumstance to which in all statistical inquiries attention should be carefully drawn, the effect namely of selection, conscious or unconscious, in affecting the result. Is it certain that the female crania which have been at the disposal of anatomists for examination have come from the same classes (or in the same degree from those classes) as the male crania? Are there not reasons for thinking that, on the whole (and in considering averages this would be a sufficient objection), the women whose crania have been examined belonged to lower classes than the men?

We note these points, not that we would confidently deny the asserted superiority of male crania, on the whole, in capacity and shape. Such superiority may exist, but may indicate no original difference of capacity. Broun Sequard has shown how the brain grows with use; and it is certain that existent systems of education give, on the whole, far less exercise to feminine than to masculine brains. In America, where women are more fairly treated, the customary tests of capacity show by no means that degree of masculine superiority which might be inferred from M. Delaunay's reasoning. On the contrary, the feminine brain holds its own so well against the masculine as to suggest the thought that had equal chances been given for as many centuries as years, the superiority might be quite the other way. Nor has this been the case only during the earlier stages of education; but often up to the final examinations.

In passing, we may note that at present it can hardly be said that the frontal lobes of the brain have been proved to preside definitely over the intellectual, while the occipital lobes preside over the life of sentiment. The animals whose brains have been experimented on do not exhibit so clearly the respective action of intellectual and sentimental ideas, that the different functions of parts of the brain can be thus localised. *Post-mortem* examinations are confessedly unsatisfactory. The relation assumed by M. Delaunay without any doubt or scruple most probably exists, but it has not yet been demonstrated.

To the objection that the intellectual inferiority of women, assuming it to exist, is due to the fact that for centuries she has not received the same education as man, M. Delaunay replies that it is unsound. "In past centuries

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the mass of the people were sunk in ignorance; neither the one sex nor the other received any education." He therefore maintains, with Professor Bischoff, of Munich, "that women have not had to the exercise and evolution of their brains any other hindrances than those proceeding from their constitution and their capacity for development." On this point he uses Cyril's argument:—

> "They hunt old trails," said Cyril, "very well;
> But when did woman ever yet invent?"

"Female musicians receive," he says, "the same education as males, and yet it is well known that, though there have been some excellent female performers, there is no instance of a great female composer. It is the same with painters and with the culinary art; among the thousands of women who have exercised the latter, there have been few, if any, *cordons bleus*. If we prepare a list of the men and another of women most distinguished in poetry, painting, sculpture, science, and philosophy, each containing a dozen names, the two lists would not bear any comparison." He cites the opinions of manufacturers and commercial men who employ individuals of both sexes. "They all agree that women are more assiduous, but less intelligent, than men. In printers' establishments, for example, women work with minute care, mechanically, without knowing very well what they are doing. Thus they make good compositors in the case of reprints, a work not demanding much intelligence, but set up manuscripts badly, not understanding them so well as men." To which it may be replied that as yet the capacity of women for such work has not been fairly tested. Miss Emily Faithfull asserts, however, that well-trained female compositors show as much readiness to deal with manuscripts as the best male compositors.

M. Delaunay touches, indeed, on the moral qualities of men and women, referring to the many authors who have maintained that women are more prone to every kind of wickedness than men, though he admits they commit proportionately fewer crimes. Poisoning is more favoured by women—that is, the poisoning of others—than by male criminals. "Moralists have noted that woman is more playful, more changeable, more capricious than man. She is also more destructive and less circumspect. The number of women run over in the streets is greater than that of men." *Quorsum hæc tam putida tendunt?* What has all this to do with M. Delaunay's subject? The argument which Mr. Delaunay uses to clinch his case, supplies the best answer to this part of his reasoning. "All known legislators take for granted the intellectual inferiority of the feminine sex as compared with the masculine. Everywhere woman is regarded as a minor, incapable of taking care of herself, and requiring a guide and tutor." The laws, in fact, having been made by man, assert his superiority, and so far as they can, ensure it. Woman is carefully placed in an inferior position, and then assured that she is an inferior being.

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COMETS' TAILS.

By the Editor.

FROM what we have already seen, it will be manifest that the formation of comets' tails is a process of a very marvellous nature, as apparently involving forces other than those with which we are acquainted. The tail, ninety millions of miles in length, which was seen stretching from the head of Newton's comet nearly along the path which the retreating comet had to traverse (the comet thus passing away with its tail in front, instead of behind, as when it approached the sun), must, it would seem, have been formed by some force far more active than the force of gravity. The distance traversed by the comet in the last four weeks of its approach to the sun under gravity was no greater than that over which the matter of the tail, seen after the comet had circled around the sun, had been carried in a few hours. Yet we have no other evidence of any repulsive force at all being exerted by the sun—at least, no evidence which can be regarded as demonstrative—and still less have we any evidence of a repulsive force exceeding in energy the sun's attracting power.

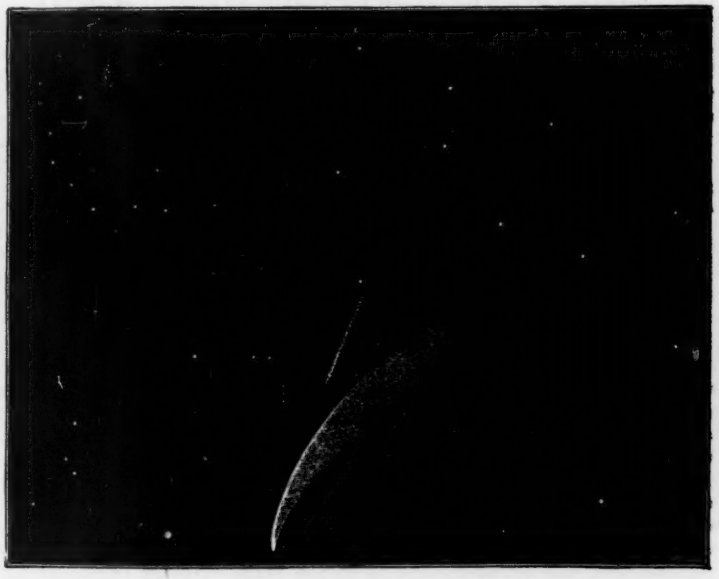


Fig. 1.—Donati's Comet, September 24, 1858.

This difficulty, and the circumstance that a comet's tail lies in the direction opposite to the sun, or in the position which the shadow of the head would occupy, has led many, unfamiliar with the laws of optics, to suppose that the comet's tail may be simply the track of the luminous rays which have passed through the comet's head. They seem to think that the head may act in some way to send a beam of condensed light along the region opposite to the sun. It should hardly be necessary, however, to explain that no such beam of light could ever be seen where we see the comet's tail. The cases supposed to correspond with the formation in this way of the tail-like appendage are, in reality, of an entirely different kind. Thus, when we see a long beam extending from a bright light, we find that first the light has been caused to pass in that direction only (as when light is admitted into an otherwise darkened room through a hole); and secondly, there is matter along

the course of the light to be illuminated. The beam is simply that long array of material particles which the light illuminates while leaving the particles in neighbouring space in darkness. So understood, such a beam is seen to be utterly unlike a comet's tail; for, in the first place, we know of no matter behind the head to be illuminated; and, in the second, we know that light is falling on the regions all around the apparent array of illuminated particles, so that these surrounding regions should be as brightly lit up, which is not the case.

If any further doubt could remain as to this theory, it would be removed by—first, the circumstance that the tail of a comet is generally curved; and, secondly, the existence of several tails extending from the head of one and the same comet.

Professor Tyndall started a theory based on physical experiments, and otherwise in better accordance with scientific possibilities. Having found that certain gases, even in an exceedingly attenuated form, form a luminous cloud under the action of the electric light, he suggested

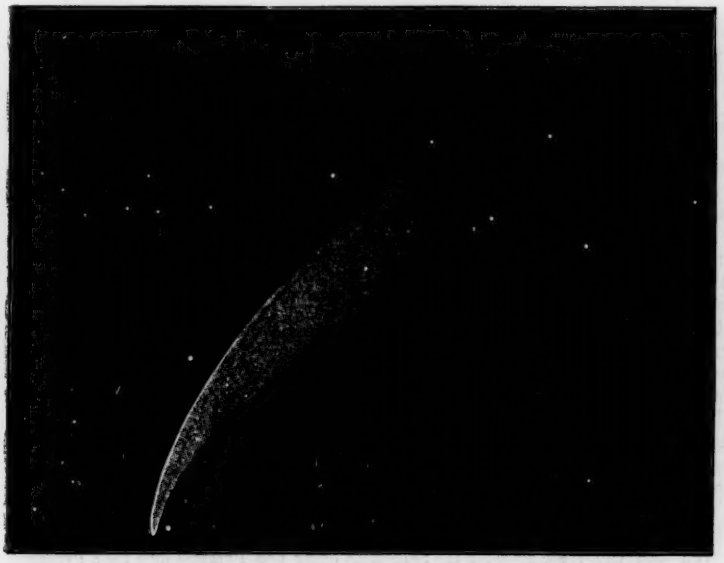


Fig. 2.—Donati's Comet, September 26, 1858.

that a comet's tail may be a luminous cloud of this sort, formed in the æther of space by those rays of sunlight which have passed through the comet's head. The rays which, without passing through the head, fall on the æther of space, would not call into existence this visible cloud, because their heating action would destroy what their chemical or actinic action *by itself* would produce. And as fast as, by the comet's motion, the cloud formed behind the head came under full solar action, it would be destroyed. So the tail would always be behind the head.

It appeared to Professor Tyndall that the curvature of a comet's tail, or the existence of more tails than one, as in Donati's Comet (Figs. 1 and 2), was not inconsistent with this interpretation. For he noticed that, according to the gas dealt with, the luminous cloud would take a longer or shorter time in becoming visible. And he suggested that when the cloud formed slowly, the tail would be curved, the part near the head being behind the position which the head had recently passed through, while the part near the end of the tail would be behind the regions through which the comet had passed much earlier. Such luminous trails as were formed more quickly would account, he considered, for the straighter tails. He overlooked, I think, the circumstance, that the shape of the luminous cloud-trail would not in reality depend at all upon the length of time which the cloud might take in becoming visible. Light would pass with the same velocity through the different kinds of tenuous gas, and whether the cloud became visible at once along the space thus passed through, or did not become visible for several seconds, or minutes, or even hours, it would become visible at the farther end of its course only just so long after it had become visible at the nearer end, as light had taken in traversing the length of cloud so formed. This interval of time would be the same for the quickly-appearing as for the slowly-appearing luminous cloud, and there would, therefore, be no difference between their forms. It would be necessary to account in this way for the curvature of the larger tail in the figure, as compared with the straightness of the smaller tails, that the curved tail should have been more slowly extended from the head; whereas the theory gives the same rate of extension for both, namely, the rate at which light travels.

We seem almost forced, by the phenomena of such a comet as Donati's, to the theory of the actual repulsion of matter from the head of the comet into the tails—matter repelled most swiftly forming the straighter tails, while matter repelled more slowly, and seemingly in greater abundance, forms the great curved tail.

We shall proceed to consider in our next paper the evidence which seems to show that, strange though this theory of material repulsion may be, it is in point of fact the only admissible theory. If this shall be established, we shall have to admit the existence of a repulsive force, whose action on the grosser material of planetary bodies is insensible.

"All knowledge, and wisdom which is the seed of knowledge, is an impression of pleasure in itself."—*Bacon.*

The Population of the Globe.—According to MM. Behm & Wagner's *Bevölkerung der Erde*, Europe has now a population of 315,929,000 inhabitants, Asia 834,707,000, Africa 205,679,000, America 95,405,000, Australia and Polynesia 431,000, the Polar regions 82,000, giving a total of 1,455,923,000, being an increase of 16,778,000, according to the latest known censuses. At the end of 1877 Germany had a population of 43,943,000, Austria and Hungary (1879) of 38,000,000, France (1876) of 36,900,000, Turkey in Europe of 8,860,000, Russia of 87,900,000. In Asia, China possesses 434,900,000 inhabitants, Hong Kong 130,144, Japan 34,300,000, according to the census of 1878. The British possessions in India number 240,200,000 people (an estimate made before the census of this year), the French possessions 280,000, Cochin China 1,600,000, the East Indian Islands 34,800,000, the islands of the South Sea 878,000. The area of Africa is estimated at 29,383,000 square kilomètres, divided as follows:—Forests and cultivated land 6,300,000, savannahs 6,235,000, steppes 4,200,000, deserts 10,600,000. The inhabitants of British North America number 3,800,000, of the United States 50,000,000, of Mexico 9,485,000, and of Brazil 11,100,000. The Polar regions extend round the Arctic Circle with an area of 3,859,000 square kilomètres, and the Antarctic regions about 600,000. The population of the former is small, with the exception of Iceland, which has 72,900, and Greenland 10,000.—*Times.*

XUM

Reviews.

IN Mr. Paterson's "Studies in Life,"* he presents a series of lectures delivered to the members of a Young Men's Christian Association. His subject is full of interest, and his book is interesting, and, on the whole, well written; but it would have been much more interesting, and, as a literary work, it would have been far better, if he had forgotten that he was addressing a religious body, or (which comes to much the same thing) if he had remembered that he was speaking about science. Mr. Paterson seems to think that he must state nothing which he cannot prove to be in exact "accordance with what we read in the Book, and what we might expect from the narrative we have there." It is true that, after starting on this principle, which for a student of science is an illogical one, he is careful to discover an accordance on a plan of his own, and then to say that it is not because such and such theories oppose the Bible, but because they are inconsistent with facts, that he rejects them; but he only makes his position more illogical still by this most transparent device. The science writers of a hundred years ago were wiser in their generation. They said (we quote from the "Encyclopædia Britannica," 1778): "This opinion, however plausible, we are not permitted to adopt, being taught a different lesson by Revelation . . . . we cannot doubt of the authority of Moses." If Mr. Paterson were content to do this, his position would be as logical as theirs. But, after expressing in effect the same opinion, he proceeds to argue the matter out, as though he were in doubt of his position. All the space thus occupied is simply wasted; and the reasoning can hardly fail to be as offensive to those who accept the authority of the Bible unquestioningly, as it is to those who cannot see what place Bible references can possibly have in scientific treatises. Mr. Paterson should know, every real student of science should remember, that science is of no creed as it is of no country. A writer of science has no more occasion to show that the science he teaches accords with his or any one else's interpretation of any religious book, than the tailor has to show that the clothes he makes are on a pattern accordant with Christian, with Mussulman, or with Buddhist doctrines.

Hence we must limit our praise of Mr. Paterson's work to the remark that if one-fourth were removed and the price proportionately reduced, it would be a work which students of biology would find worth getting and reading. With the fourth referred to would go much false science. We venture, too, to say that the interests of religion are, to say the least, not advanced by such passages as we refer to. It cannot conduce, for example, to the reverential spirit Mr. Paterson inculcates, to read a paragraph,—beginning with the statement that Darwin's theory is essentially atheistic, going on to refer to what Mr. Paterson knows "in his inmost soul by the revelation of the Holy Ghost"; and closing with the statement that, even if the Bible does not forbid, hybridity sets up an impassable barrier.

In "Health Studies" † and "The Human Body," ‡ Mr. Paterson appears to better advantage, though he loses even here no opportunity of making science and religion simultaneously ridiculous.

Yet all three works are full of interesting and, for the most part, instructive matter; and but for the serious defect we have pointed out, they might all three be strongly recommended. Albeit, there is another fault—to wit, an affectation of simplicity, a very different thing from real simplicity. No one could write more simply than Faraday, yet his simplest words never wanted dignity, and never offended his reader's sense of self-respect: when we find our author speaking of the features of the face as Mouthgate, Nosegate, and so forth, and gravely telling us that the nose is useful, "even though it may also be ornamental," and the like, we cannot accord the same praise to him.

"THE FIJI ISLANDS."—We omitted to notify in the footnote to our review of Mr. Horne's work on "The Fiji Islands," that it is published by Mr. Edward Stanford, Charing Cross.

* "Studies in Life," by H. SINCLAIR PATERSON, M.D. (London: Hodder & Stoughton.)

† "Health Studies," by the same author.

‡ "The Human Body," by the same author.

THE GREAT PYRAMID MEASURES, AND THE DIAMETERS AND DISTANCES OF THE SUN, EARTH, AND MOON.

BY JOSEPH BAXENDELL, F.R.A.S.

[IT must not be understood that we accept our esteemed contributor's views. They illustrate well the whole theory of pyramid coincidences, but these coincidences disprove, in our opinion, what Mr. Baxendell considers that they prove.—ED.]

A FEW months ago the results of a partial discussion of the Great Pyramid measures, given by Professor C. Piazzi Smyth, in the fourth edition of his work entitled "Our Inheritance in the Great Pyramid," led me to believe that the data which had formed the basis of the design for the Pyramid were the diameters and distances of the sun, earth, and moon, combined with the ratio (π) of the circumference of a circle to its diameter—a quantity which forms an important feature in the relations of the Pyramid measures; and, also, that in order to reduce the results of the astronomical data to magnitudes suitable for the design and construction of the Pyramid, a scale of one pyramid inch to a length, one-thousandth part greater than the present English mile, or 63,360 pyramid inches, had been used by the architect; but as I found that the values of the diameters and distances given in various astronomical works, especially those for the diameter and distance of the sun, would not yield results agreeing *exactly* with the Pyramid measures, although they were generally remarkably close approximations, I was induced to undertake a more extended discussion and analysis of the measures, with a view to ascertain, if possible, the exact values which had been employed by the architect in his reductions, and it thus became necessary to attempt a solution of the following problem. Given approximate values of the diameters and distances of the sun, earth, and moon to find the values which in simple combinations will give, with *strict exactness*, the various Pyramid measures and numbers, the scale for the reductions being one Pyramid inch for a Pyramid mile of 63,360 Pyramid inches. For some time I had considerable difficulty in forming the requisite number of suitable equations for the complete solution of this problem, but ultimately succeeded, and obtained the following values:—

	Pyramid miles.	English miles.
Diameter of the Sun	855,938	856,793
Equatorial diameter of the Earth	7,917·7	7,925·6
Diameter of the Moon	2,157·2	2,159·3
Mean distance of the Sun.....	91,758,800	91,850,558
Mean distance of the Moon	238,483	238,721

Let S = distance of the sun; M = distance of the moon; s = diameter of the sun; e = equatorial diameter of the earth; m = diameter of the moon. Then the following equations, in which pyramid miles and inches are adopted, will show the relations between these numbers and the pyramid measures:—

1. $\frac{se}{m} = 1{,}000{,}000\pi$.

It is probably owing to the remarkable relation in the magnitudes of the three bodies shown by this equation that the quantity π forms so prominent a feature in the relations of the Pyramid measures.

2. $\sqrt{s\pi^2} = 9{,}131{\cdot}05$ = length of one side of the base of the Pyramid.
3. $\sqrt{s2\pi} = 5{,}813{\cdot}01$ = height of the Pyramid.
4. $\frac{s\pi^2\pi\sqrt{\pi}}{25{,}000} = 1{,}881{\cdot}59$ = length of Grand Gallery.
5. $\frac{2\sqrt{s\pi}\sqrt{\pi}}{25} = 412{\cdot}13$ = length of King's Chamber.

6. $\frac{\sqrt{s\pi}\sqrt{\pi}}{1{,}000} = 5{\cdot}151{,}646$ = the number which has been called the key number to the dimensions of the King's Chamber, and of the Pyramid generally.

7. $S = \frac{25{,}000{,}000e}{m}$

8. $M = \frac{em\pi}{3^2 5^2}$

9. $M = \frac{se^3}{9ms}$

10. $\frac{2S}{5{\cdot}151{,}646M} = 149{\cdot}37$ = height of ante-chamber.

11. $\frac{3\sqrt{SMs\pi^3}}{250e} = 36{,}524{\cdot}22$ = perimeter of base of the Pyramid.

12. $\frac{3\sqrt{SMs\pi^3}}{500e\pi} = 5{,}813{\cdot}01$ = height of the Pyramid.

13. $\frac{es\pi^4}{75\sqrt{SM}} = 1{,}881{\cdot}59$ = length of Grand Gallery.

14. $\frac{80e\sqrt{s\pi^2}}{3\sqrt{SM}} = 412{\cdot}13$ = length of the King's Chamber.

Among the equations I obtained during the investigation were several which gave a smaller value for the diameter of the sun; and as I am not aware that any sensible difference has ever been observed between the polar and equatorial diameters, this result seemed adverse to the theory of a connection between the Pyramid measures and the diameters of the three bodies, until it occurred to me that probably one diameter referred to the photosphere, and the other to the comparatively dark and solid or liquid body of the sun. This latter diameter is 853,718 Pyramid miles, or 2,220 miles less than that of the photosphere, and the following equations, in which it is represented by the Greek letter σ, will show its connection with the Pyramid measures:—

15. $\frac{\sigma^2\pi}{e^2} = 36{,}524{\cdot}20$ = perimeter of base.

16. $\frac{\sigma^2}{2e^2} = 5{,}813{\cdot}01$ = height of Pyramid.

17. $\frac{\sigma^2}{100e^2} = 116{\cdot}26$ = length of ante-chamber.

18. $\frac{\sigma^2\sqrt{\pi}}{50e^2} = 412{\cdot}13$.

19. $\frac{\sigma^4\pi\sqrt{\pi}}{400{,}000e^4} = 1{,}881{\cdot}59$.

20. $\frac{\sigma^{2\frac{1}{2}}\sqrt{\pi^3}}{e^2 2\sqrt{10^5}} = 1{,}881{\cdot}59$.

21. $\frac{\sigma^2\sqrt{\pi}}{4{,}000e^2} = 5{\cdot}1{,}516$.

The length of the earth's polar axis is assumed by pyramidists to be 500,000,000 pyramid inches, or 7891·41 pyramid miles of 63,360 pyramid inches to the mile, or 7,899·30 English miles, while the value derived by Col. Clarke, from an elaborate discussion of measurements of arcs of meridian, is 7,899·11 English miles—the difference being therefore, less than two-tenths of a mile. I was, therefore, much surprised to find that the Pyramid measures would not yield a less diameter for the earth than 7,892·54 pyramid miles, or more than a mile greater than the generally-accepted length of the polar diameter. The question therefore arose—Can this latter length be in error to the extent indicated, or is the value I have obtained connected in any way with some marked feature of the Pyramid? It seemed to be highly improbable, if not impossible, that the results of the calculations of Bessel, Airy, and Clarke could be in error to the extent of more than a small fraction of a mile, and assuming, therefore, that the figure of the earth is truly spheroidal with major axis = 7,717·7, and minor axis = 7,891·41 Pyramidal miles, I calculated the geo-centric latitude in which a diameter will be 7,892·54 miles, and found it to be 78° 25′ 33″; and, deducting this from 90°, we have 11° 34′ 27″. A glance at this result at once suggested that it was the polar distance of the Pyramid pole-star, α Draconis, multiplied by the quantity π, and on dividing 11° 34′ 27″ by π I obtained 3° 41′, which is a very close approximation to the calculated polar distance of α Draconis at the time of the building of the Pyramid. Now a section of the earth through the parallel of latitude marked out in so singular a manner has a diameter of 1,583·54 Pyramid miles, or exactly one-fifth of the earth's equatorial diameter, and an area of 1,969,462 miles, or one twenty-fifth that of a section through the equator, which is 49,236,600 miles. The occurrence of the Pyramid numbers 5 and 25 in connection with the diameter thus indicated in so striking a manner gives a peculiar importance to it, and accordingly I have found that expressions in which it is a factor can be formed which give *exactly* the various Pyramid measures. Thus, representing this diameter by the Greek letter η (eta), we have

22. $\eta = \frac{S\sqrt{\pi}}{4{,}000 \times 5{\cdot}151{,}646} = 7{,}892{\cdot}54$.

23. $\eta = \frac{Se^2}{\sigma^2}$

24. $\frac{S\sqrt{\pi}}{4{,}000\eta} = 5{\cdot}151{,}646$.

25. $\frac{S}{\eta} = 11{,}626{\cdot}02 = 100$ times length of ante-chamber.

26. $\frac{S^2\pi\sqrt{\pi}}{4{,}000\eta^2 5{,}151{,}646} = 36{,}524{\cdot}22$.

27. $\frac{\sqrt{2e\eta}}{100} = 111{\cdot}795$ = height of granite wainscot in ante-chamber.

28. $\frac{450\sigma^2\eta}{5{\cdot}151{,}646e^3m\pi} = 149{\cdot}37$.

29. $\frac{S^2\pi\sqrt{\pi}}{400{,}000\eta^2} = 1{,}881{\cdot}59$.

30. $\frac{S\pi 5{\cdot}151{,}646}{100\eta} = 1{,}881{\cdot}59$.

31. $\frac{S\pi}{4\pi} = 9{,}131{\cdot}05$.

32. $\frac{S}{2\eta} = 5{,}813{\cdot}01$.

It may be remarked that the diameter η is exactly one seven-thousandth part greater than the polar diameter, and that the parallels of latitude in which it occurs may be regarded as the limits of the habitable portion of the globe.

The results of my investigation having proved that a measure corresponding to our English mile, and containing 63,360 Pyramid inches, was used by the architect of the Pyramid, it became a matter of interest to ascertain, if possible, how it originated, and ultimately I arrived at the following formula:—

33. $10\sqrt{\frac{Se}{m}} = 17{,}724{\cdot}5$ miles, which is the circumference of a circle whose area is 25,000,000 miles, or equal to the area of a section of the earth through the parallel of latitude in which the length of a diameter is equal to the mean of all the earth's diameters (7,904·545 P. miles). This area expressed in Pyramid inches, is equal to a square, the side of which has a length of 316,800,000 inches, and this, divided by 5,000 = 63,360 inches.

My experience in the development of the theory which has yielded the results given in this paper has convinced me that there is no feature of the Great Pyramid, or relation of its various parts, which cannot be expressed in terms of the astronomical data I have used, and in some cases, as I have already shown, two, three, or more equations can be formed, each containing one or more factors not in the others, but giving precisely the same result. It is evident, therefore, that the builder possessed a far greater amount of mathematical and astronomical knowledge than it has hitherto been supposed could possibly have been acquired by the ordinary course of observation and scientific investigation in the early age of the world when the Pyramid was built; and the fact that the values of the diameters and distances used by him are within the limits of the probable errors of the means of the best astronomical determinations of recent times proves that, so far at least as these values are concerned, modern science has made no real advance upon the science known to the builder of the Great Pyramid 4,000 years ago.

NEW MODE OF GROWING PLANTS.

By E. C.

To Dr. C. W. Siemens, the celebrated electrician, we are indebted for a series of experiments lately tried by him on the effect of the dynamo-electric light in promoting the vegetation and growth of plants. The idea that the electric current might be utilised in this way first occurred to him by observing that the blistering effect on the skin from this light was very similar to that produced by a hot sun. Without the aid of the sun's rays, *chlorophyll* is not formed, and this is an all-important element in vegetable life, as it produces the green colour of the leaves, and supplies the plants with carbon and starch for forming woolly tissues, by causing the decomposition of the carbonic acid vapour absorbed from the atmosphere by the leaf. The electric light, being, in fact, a sun on a small scale, has been formed to produce *chlorophyll* and other necessary chemical changes in a similar way to solar rays.

XUM

The apparatus employed for the series of experiments was a small upright Siemens machine, worked by a gas-engine. The two carbons in the regulator lamp were respectively 10 and 12 millimètres in diameter, and the light it produced was equal to 1,400 candles. The first plan that was tried was by placing the lamp about 7 ft. above a melon pit, and a reflector was arranged to concentrate all the light on the sash. Pots of rapidly-growing plants were, in succession, brought under this influence, some exposed only to the electric light, some to the sun alone, and some to the sun and electric light alternately. The latter made the most rapid progress, and were the best in colour. In the next experiment, the lamp was put inside a greenhouse, and as near the roof as possible. Various plants were placed in pots on the floor, at a variety of distances, and the light was kept burning all night for one week. The plants nearest to the influence of the lamp made the most rapid progress, and the foliage and plants were of a far brighter hue than if they had only had sunlight during the day. As regards forcing fruit, the electric light seems very efficacious. In ten days time some strawberry plants, which had been kept alternately under the influence of the sun and light, had large, full-flavoured fruit, while plants which had only been exposed to the rays of the sun for a similar period, had merely green berries on them. There is no doubt that the use of artificial sunlight in horticulture will be of immense advantage; but whether it can be used by market gardeners and people supplying the London markets, entirely depends on the price at which it can be produced. The machine used by Dr. Siemens, of 1,400 candle-light, costs about fivepence an hour to work, exclusive of a man, but including the cost of carbons. Dr. Siemens is of opinion, that a light equal to 6,000 candles would prove to be economical in working. This would have to be fixed 20 ft. above the ground. For forcing early fruits and flowers for the London markets, this discovery, if not too expensive, will be invaluable, as the rays may be concentrated on a brick wall, and by this means fruit may be rapidly ripened. At a lecture at the Royal Institution, given by Dr. Siemens, the action of the dynamo-electric light was tried on some tulips, and it had the effect of causing the small buds to expand to full-blown flowers in about twenty minutes. The electric machine will apparently soon be applied to purposes of general utility, as it enables work on a heavy scale to be carried out, and it is already employed for telegraph work, superseding the voltaic battery. Should Dr. Siemens succeed in producing it in a cheap form, it would be largely adopted, without doubt, in horticulture, as the advantages of being early in the market are well known. It seems almost an undisputed fact, that plants subjected to the influence of the dynamo-electric light arrive at perfection in rather less than half the time they would have taken if left only to the rays of the sun.

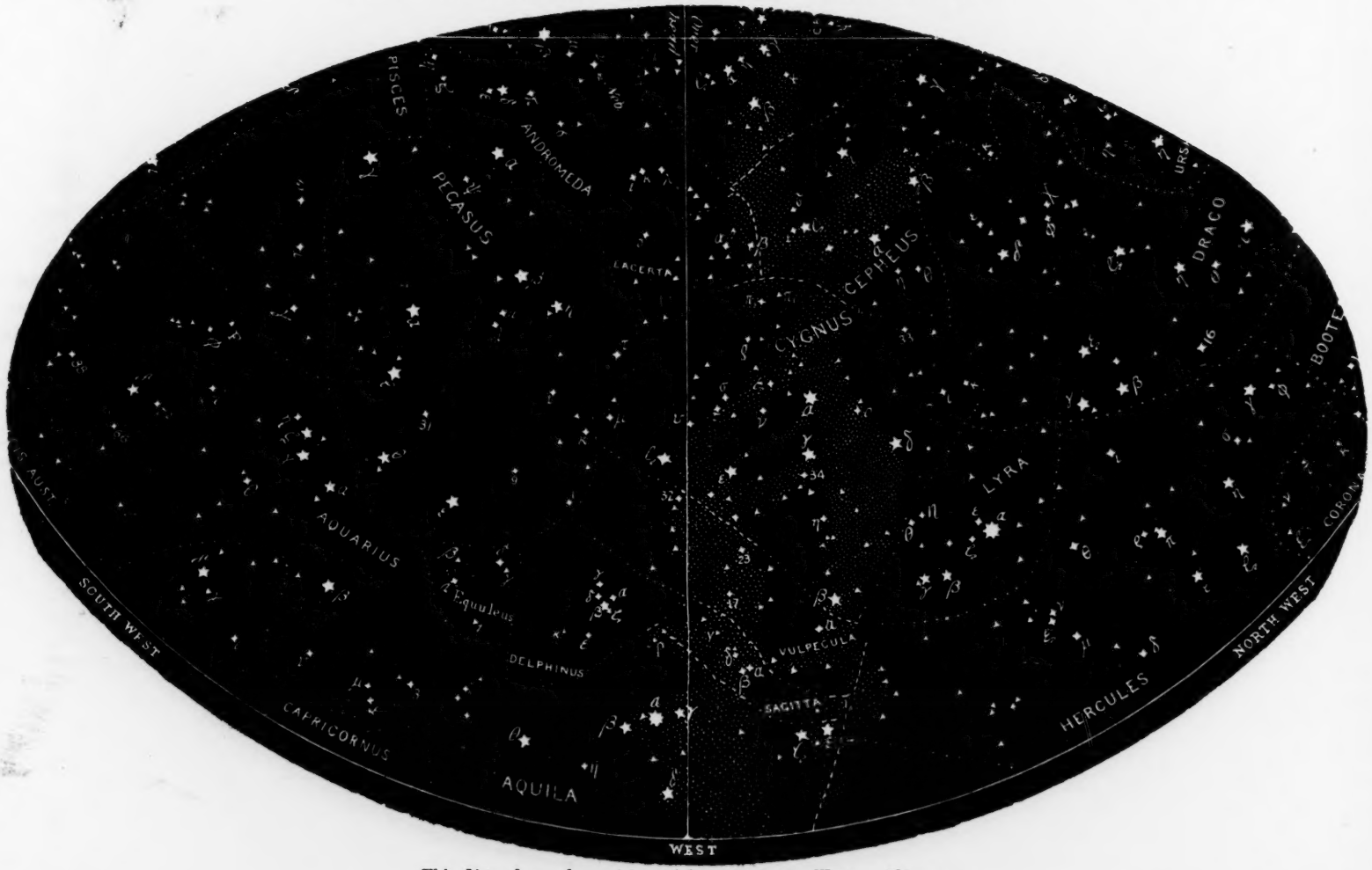
A PLANET OUTSIDE NEPTUNE.

RATHER more than half-a-century ago, astronomers were beginning to suspect that outside the path of the planet Uranus another planet must travel, whose attraction caused Uranus to deviate somewhat from the motions which theory assigned to him. We know how this idea eventually led to the discovery of Neptune. From the observed disturbance of Uranus, the place of the disturbing body was determined by Leverrier and Adams independently, and when the telescope was turned to the region of the heavens where this as yet unknown planet should be, *there*, quite close to the calculated spot, was his disc seen. It seems not unlikely that before many years pass another planet further away than Neptune will be discovered. When Professor Newcomb, of Washington, published his "Tables of Neptune" in 1866, he said, "It is almost vain to hope for the detection of an extra-Neptunian planet from the motions of Neptune before the close of the present century." But since Newcomb's "Tables of Uranus" were published in 1873 the planet (Uranus) has been found to pass increasingly more in advance of its calculated place. Mr. D. P. Todd, Assistant in the Nautical Almanac Office at Washington, has found that by a graphical method (the problem not being yet in such a condition that processes of calculation can be applied), that the position of a disturbing body outside the orbit of Neptune can be approximately inferred. He places this body at a distance from the sun exceeding the earth's fifty-two times, so that its revolution around the sun would require 375 years. Its present position along (or near) the ecliptic would be within ten degrees either way of longitute 170°, so that it might be looked for ten degrees along the ecliptic, on either side of the star Tau Leonis. Assuming the distance correctly taken (as in the case of Neptune, the real planet may be at a much greater or at a much less distance than the hypothetical one, without greatly affecting the result as to the disturbing body's apparent position), the mass would be such that, assuming the density similar to that of the four giant planets, the apparent diameter would be about 2″·1 (say about the 900th part of the moon's), and it would appear as a telescopic star of about the thirteenth magnitude. He puts the point where the planet's path crosses the ecliptic in longitude 103°, and the inclination to the ecliptic as 1° 24′. By the kindness of Rear-Admiral Rodgers, superintendent of the Washington Observatory, Mr. Todd was allowed to begin the search for the trans-Neptunian with the magnificent 26-inch telescope. A power of 400 was employed, which would make the diameter equal to nearly half the moon's, and should therefore show the disc very obviously. On thirty clear moonless nights Mr. Todd searched along the neighbourhood of the ecliptic from longitude 126° 8′ to longitude 186° 1′, without leaving any unsearched space between these longitudes. He says, "if a trans-Neptunian planet is ever discovered, having a diameter as great as 2″, I shall be very much surprised if it is found that it must have eluded my search." But he suggests that at and about the time of the next opposition (which, if his assumed position is correct, would occur within ten or twelve days on either side of March 20), a new search should be made, with a telescope of sufficient power. If a careful and sufficient search near the indicated longitude should prove unavailing, it would be necessary to extend the observation to a limited zone, all round the heavens. It is not likely that the inclination of any trans-Neptunian planet to the ecliptic would be more than 2°, so that an ecliptical zone 4° wide would probably suffice for survey.

Reports of Societies.—We have been requested to insert reports of the meetings of various societies. But the papers read before scientific societies are, as a rule, unsuited to our columns, and we prefer (for the present at any rate) to give our space, which is limited, to matter not requiring translation before it can be understood by the general reader. We shall be very glad to find space for clear and simple accounts by the authors of papers communicated to societies, of the matter which they properly enough present in technical terms to fellow workers. And when papers are of sufficient interest, we shall present our readers with translation specially drawn up for these pages. But reports of scientific societies as usually prepared, that is condensed versions of statements too technical even without condensation to be generally understood, would be simply a waste of space in a journal expressly intended to be of interest to the general public. Moreover, a large amount of matter communicated to learned societies has no interest (even when explained) to other than experts. Our wish is not that all such matter as is thought suitable for communication to scientific societies should appear in these columns (by a sort of editorial pitchforking), but that whatever is worth knowing outside those societies may be sifted out and placed, when duly translated, before our readers. The former course would be the easier and the cheaper; the latter seems the more honest, as alone in accordance with our programme.

Indiarubber Gathering in Columbia.—An interesting account is given of this process in a report just issued by the United States Consul at Carthagena. When the hunter has found a rubber tree, he first clears away a space from the roots, and then moves on in search of others, returning to commence operations as soon as he has marked all the trees in vicinity. He first of all digs a hole in the ground hard by, and then cuts in the tree a V-shaped incision, with a machete, as high as he can reach. The milk is caught as it exudes and flows into the hole. As soon as the flow from the cuts has ceased, the tree is chopped down, and the trunk raised from the ground by means of an improvised trestle. After placing large leaves to catch the sap, gashes are cut throughout the entire length, and the milk carefully collected. When it first exudes, the sap is of the whiteness and consistence of cream, but it turns black on exposure to the air. When the hole is filled with rubber it is coagulated by adding hard soap, or the root of the mechvacan, which have a most rapid action, and prevent the escape of the water that is always present in the fresh sap. When coagulated sufficiently, the rubber is carried on the backs of the hunters by bark thongs to the banks of the river and floated down on rafts. The annual destruction of rubber trees in Columbia is very great, and the industry must soon disappear altogether, unless the Government puts in force a law that already exists, which compels the hunters to tap the trees without cutting them down. If this law were strictly carried out there would be a good opening for commercial enterprise, for rubber trees will grow from eight to ten inches in diameter in three or four years from seed. The trees require but little attention, and begin to yield returns sooner than any other. Those that yield the greatest amount of rubber flourish on the banks of the Simu and Aslato rivers. The value of the whole indiarubber imported into the States annually is about $10,000,000.

THE WESTERN SKIES IN NOVEMBER.



This Map shows the position of the stars in the Western Skies:—

On November 3, at 11¼ o'clock.	**On November 10, at 10¾ o'clock.**	On November 18, at 10¼ o'clock.	On November 25, at 9¾ o'clock.	On December 3, at 9¼ o'clock.
On November 7, at 11 o'clock.	**On November 14, at 10½ o'clock.**	On November 22, at 10 o'clock.	On November 29, at 9½ o'clock.	On December 7, at 9 o'clock.

The stars in the western skies are passing downwards towards the horizon, moving aslant, from left to right; but those shown on the right or northern side of the map will not set, but pass over towards the north, there to pass below the pole.

***** Stars of the first magnitude are shown with eight points, those of the second with six, of the third with five, of the fourth with four, of the fifth with three.**

XUM

Letters to the Editor.

[*The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. He requests that all communications should be as short as possible, consistently with full and clear statements of the writer's meaning.*]

All communications should be addressed to the Editor of KNOWLEDGE, *74, Great Queen-street, W.C.*

All Cheques and Post-Office Orders to be made payable to Messrs. Wyman & Sons.

*** *All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

ERROR IN COMPUTING PERIPHERY OF ELLIPSE.

[14]—Will you pardon me for pointing out a slight error on page 37 of KNOWLEDGE?

In the reduction of the expression

$$7\pi[1-\frac{1}{4}.\frac{13}{49}-\frac{1}{64}.\frac{(13)^2}{(49)^2}\ \&c.]$$

You give the result as 20·58 in, instead of which it should be 20·508 in. I give the details as follows:—

$$7\pi[1-\frac{1}{4}.\frac{13}{49}-\frac{1}{64}.\frac{(13)^2}{(49^2)}]$$

$$=7\pi[\frac{64.(49)^2-16.49.13-(13)^2}{64.(49)^2}]$$

$$=7\pi[\frac{153664-10192-169}{153664}]$$

$$=21.\ 99912.\ \frac{143303}{153664}$$

$$=20{\cdot}508.$$

The solution by logarithms gives a similar result.

Yours faithfully,

WILLIAM J. HARDING.

P.S.—May I suggest that the above expression may be given as an example worked out by logarithms to show their great convenience?—W. J. H.

[Mr. Harding is quite right. Turning to my computation, I find logarithm of result given correctly as 1·3119321, which is the logarithm of 20·50841. I took out the number, however, incorrectly. The difference between the circumference of an ellipse having axes 7 and 6, and the circumference of a circle having diameter 6½, is only a tenth of what I deduced, and is in fact less than the hundredth of an inch. Had I taken axes less nearly equal, there would have been a greater difference; but it is only when the very irregular heads are considered that the difference arising in this way can be worth taking into account.—ED.]

COMETS.

[15]—I have read with interest the article on comets. I am always on the look out for astronomical news, and turned to the article in question in the hope that some light might be shed on the physical nature and purpose of these mysterious tourists in space. I have long been expecting some definite theory to be broached to account for them; some facts have been collected, but comets are still, apparently, without the pale of celestial civilisation and order. The connection between them and meteor streams is established. Are we, then, to regard meteor streams as the condensed material of comets left behind, and separated from the main body in the course of its revolution round the sun? If so, how is the fact accounted for that spectroscopic examinations shows that comets are mainly composed of the vapour of carbon, while the meteorites that have been examined generally contain a large proportion of iron, nickel, &c.? Is it possible that a flight of meteors at a great distance might present the appearance of a comet? Are comets to be regarded as a primary condition of matter, to be afterwards condensed into meteors, and these in turn to assist in the formation of new planets, and the gradual increase of old ones? I believe the theory, first put forth by the editor of KNOWLEDGE, that the rings of Saturn* are composed of a multitude of minute satellites, is generally accepted. Would it be in accordance with that theory to go a step further, and consider the rings to be dense meteor streams? and if so, might they have been introduced to Saturn's system by a comet or comets? Any information on these points will, doubtless, be acceptable to others besides

ONE WHO WANTS TO KNOW.

P.S.—The crape ring of Saturn was well seen here (Hastings) on the evening of Nov. 6, power 250, 4¼ in. refractor.

FIGURES OF THE CONIC SECTIONS.

[16]—When I am studying geometry, as in Euclid, I am able to describe circles of any size and in any position with compasses. But in studying the geometry of the conic sections, I am not able to make such illustrations as I want. I cannot draw the parabola or the hyperbola freehand; nor, indeed, can I draw a satisfactory ellipse in this way. But, even if I could, I want something more. I want to be able to draw with exactitude a parabola, hyperbola, or ellipse, as occasion may require, in any position and of any size or shape (parabolas, of course, are all of the same shape, but hyperbolas and ellipses are not). I observed a few weeks since in a back number of the *English Mechanic* what seems to me a very simple and satisfactory way of drawing parabolic and hyperbolic arcs, as follows:—

For the *parabola*, set two straight lines AB, BC, (Fig 1) at right angles to each other, and divide each into the same number of equal parts in the points 1 2 3 4 5, join A1, A2, A3, A4, A5, and through

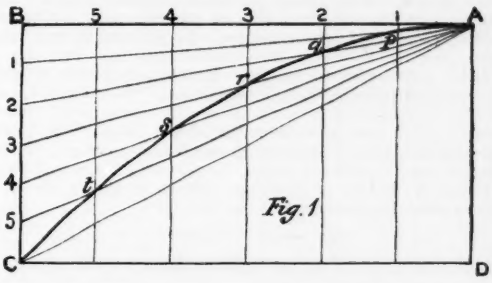


Fig. 1

the points of division along AB draw parallels to BC. Then the points *p*, *q*, *r*, *s*, *t*, in which the parallels through 1, 2, 3, 4, 5 meet A1, A2, A3, A4, A5, lie on a parabolic arc through AC, which can be swept out as in the figure.

For the *hyperbola*, set two straight lines AB, AC (Fig. 2) at right angles, and from a point O outside BAC draw through any points 1, 2, 3, 4 on AB, straight lines cutting AC in 1, 2, 3, 4. Through

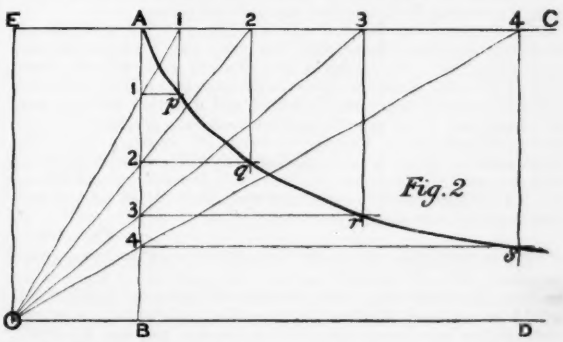


Fig. 2

11, 22, 33, 44 draw parallels to AB, AC, meeting in *p*, *q*, *r*, *s*, then the points A *p*, *q*, *r*, *s* lie on a hyperbolic arc which can be swept out as in the figure.

This is well, so far as it goes; but it does not meet my difficulty.

* [The theory was first advanced by the Bonds, in America, and independently by Clark Maxwell, of Glasgow.—ED.]

We get in this way a parabolic and a hyperbolic arc (the ellipse cannot be represented at all in this way by the simple use of parallel straight lines), but we have no means of drawing a parabola or hyperbola of given size, having a given focus and directrix, or in the case of the hyperbola, having a given centre and given asymptotes. I am aware that there are mechanical means, some of which seem simple enough, for drawing all the conic sections, but they do not suit my purpose. I want to be able at any time, with the instruments found in an ordinary box of instruments, to draw a parabola, hyperbola, or ellipse of determinate size, position, and shape. Can you help me to meet this difficulty?

GEOMETRICUS.

[I will take an early opportunity to describe some simple methods of drawing the conic sections in the way required by Geometricus. The methods described in his letter are correct; but in practice it is well to have methods which give a series of enveloping lines, guiding the hand in sweeping out the curves after such points as p, q, r, s, and t have been obtained; or the curve may be struck out in that way without obtaining determinate points as above. To illustrate my meaning, let Geometricus join C 3 in Fig. 1, which line will be trisected where crossed by p 5 and s 4. Join these points of trisection with the points 2 and 1 (on A B) respectively; then the two straight lines thus drawn and C 3 will touch the parabolic arc at C, s, and q respectively. With A B there will be four tangents. If A 3 and C 3 (3 on A B) be divided into six equal parts, and the successive points of division along C 3, be joined with the successive points of division along 3 A, there will be obtained seven tangents touching the arc A C at A, p, q, r, s, t, and C. Tangents for the curve A, q, s, Fig. 2, may be obtained in another way, which I will describe, as also methods for drawing ellipses, either by obtaining any number of points along the curve, or by getting a series of tangents enclosing it. In the meantime, I note that A is the vertex, AD the axis of the parabolic arc AC in Fig. 1; neither its focus nor its directrix is given directly by that method, but by taking BC bearing a definite ratio to AB, the position of the point which is at the extremity of the latus rectum (or focal chord perpendicular to the axis) can be determined at once, and thence the position of the focus and directrix. In Fig. 2, A q, is part of one branch of a rectangle hyperbola, of which O is the centre, and OD, OE are asymptotes. The point A is not a determinate point on the curve unless the rectangle OA is also a square, in which case A is the vertex of the branch. By making OA a rhomboid, a hyperbolic arc having its asymptotes inclined at any angle, can be obtained; and if the rhomboid is also a rhombus, the vertex of the arc so described will be at that angle of the rhombus which lies farthest from O.—ED.]

CAN ICE-YACHTS SAIL FASTER THAN THE WIND?

[17]—"Upsilon" (letter 3, p. 16) is one of the many and intelligent men who have been perplexed by this apparently simple question. I think the following considerations appeal to the judgment, perhaps, more forcibly than those you append to "Upsilon's" letter:—

If a ship is sailing before the wind, a pressure is manifestly exerted on its sails, and in consequence, the velocity increases until the ship has the same velocity as the impelling fluid (the air). This is the theoretical limit, for then the pressure ceases.

But if, on the other hand, a ship is sailing with the wind abeam, no matter how great the velocity, the moving air exerts a pressure on the sails. The component of this pressure resolved in the direction of the ship's motion tends to increase that motion, and since the wind pressure is constant in action and direction, the ship may be considered to be moving under a uniformly accelerating force. Hence, if there be no drifting to leeward (of course, a theoretical consideration), there is no theoretical limit to the velocity which the ship may attain. In the case of ice-yachts, the drifting and friction are at a practical minimum, and the speed they attain may be very much greater than that of the wind.

CRUSADER.

[The reply we gave last week takes the line which "Crusader" suggests. He will see, however, that the accelerating force is not uniform, but diminishes as the velocity of the ship increases. It would be a pretty problem to determine, with certain necessary assumptions as to sails, frictional resistance, &c., the maximum velocity attainable with a given wind.—ED.]

IS THE SUN HOT? (*Abstract.*)

[18]—The sun is regarded as the fountain head of all terrestrial energy. The gravitation of the central mass of the sun causes tremendous compression, giving birth to the forces that are transmitted to us. Now the forces that we are most sensible to are heat and light, but there is another force that we are not so sensible to, *i.e.*, chemical force. Did "Anti-Guebre," when he drew near to his fire, ask himself the following question: Where did the heat originate? The answer is: The sun. For the sun transmitted his energy (by the medium of the æther) to the earth, the force was utilised by plants, plants in course of time changed into coal.

Now, as regards the "mountain proposition." It is a well-known fact that the "rays" of the sun pass *through* the atmosphere without materially altering its temperature, and are absorbed by the earth, which gives out again the heat which it has absorbed to the surrounding atmosphere. Now, on a mountain or in a balloon, we are further from the actual source of heat, and the air, being thin and rarefied, does not absorb heat like the denser and nearer-to-the-earth atmosphere.

The explanation of the mountains, &c., will also apply to the cirrus clouds.—Yours, &c. SUN.

[19]—With regard to Mr. Newton Crosland's letter (No. 5, p. 35) on this subject, which is after all mere supposition, surely it is more natural to suppose that a body which produces all the phenomena of heat is in itself hot—at all events, till we find more evidence to the contrary than N. C. can furnish. He says the inflammatory action may be merely the chemical conversion of substance into force. I should like to know what is combustion but this? Your correspondent "Tyro" seems under the impression that light is visible; perhaps when he hears it is not, he will be able to account for the non-appearance of the broad flood of effulgence.—Yours, &c., C. J. SHAW.

[20]—I may, perhaps, be permitted to say a word in opposition to "Anti-Guebre's" views.

Before discussing any point, I shall at once state that I take the "conception of a medium filling space, and fitted mechanically for the transmission of vibrations of light and heat"—in other words, the *luminiferous æther*, as the foundation of my remarks. "Anti-Guebre" leaves the subject of radiation and absorption untouched, and here I think we have the explanation of his observations.

The conception of the æther filling space may now fairly be said to be inductively proved. Prof. Tyndall's experiment, in which he allows heat waves from a radiant body to pass through a glass tube, taking the temperature by the thermo-electric pile, shows, beyond a doubt, that *air* is incapable of absorbing heat.

He says "oxygen, hydrogen, nitrogen, and the mixture, atmospheric air, prove to be practical vacua to the rays of heat." Waves of heat, then, travel from the sun to us without having been absorbed; but directly they impinge upon the earth, and upon absorbing and radiating bodies, they produce heat, and cause a brisk radiation. Taking the accepted scientific definition of heat as "a brisk agitation of the parts of an object," we can easily understand that when the atoms of the æther "swing" with the atoms of the bodies upon which they impinge, a quicker movement takes place, and consequently heat is generated. Absorption, then, is a source of heat, and heat so generated is radiated into the air and turned to account. The power of the atmospheric aqueous vapour to absorb heat *radiated from the earth* is immense, and it is calculated that 10 to 15 per cent. of heat from the earth is absorbed within 10 or 20 feet of the earth's surface. It has been observed that where the air is dry, and remarkably free from aqueous vapour, as in Australia, the temperature of the night is 50° to 60° below that of the day, because there is no check to radiation. Anti-Guebre's, and all observations fall in with these explanations, and prove them. The further we go from the earth's radiation, the colder we get. It is a remarkable fact, proved by experiment, that aqueous vapour has the power to absorb rays of heat coming from the *earth*, but is incompetent to absorb rays from the *sun*.

By this remarkable adjustment the earth is rendered habitable.

G. F. P. DYER.

1, Queen-square, Bath.

WASTE OF SOLAR HEAT.

[21]—The letter in your present issue touches on a point in which it has long appeared to me that scientific language is erroneous. We talk of "Heat" coming from the Sun; and under that impression, "Anti-Guebre's" complaint of the "loss of Heat" has some plausibility. But it appears to me that the energy radiated by the Sun is only one factor of the result called Heat.

Energy	+	or	×	Something	=	Heat
Energy	+	or	×	Something else	=	Light.
Energy	+	or	×	Something else	=	Electricity,

and Heaven knows how many other yet unknown powers. There-

fore, it is by no means certain that this energy should be wasted, although it fails to (*i.e.*, is not designed to) form heat on this little globe.—COGITO.

A CLEVER SEA-GULL.

[22]—During a rec nt passage over the North Sea, a flock of sea-gulls followed the steamer for many miles. At last I noticed that one of them, a remarkably fine bird, had, by some chance, got an angler's line attached to its wing. The poor thing flew about the rigging, its companions, meanwhile, uttering loud cries. After great cawing, the bird flew quickly towards the ship, dashed round one of the ropes several times, and ultimately flew off, leaving the line twisted round the rope. Was this what Mr. Herbert Spencer would call "reason in inferior animals?"

Hoping to see your journal succeed, as it deserves to do,

I remain, yours,

GEO. B. FRASER.

Helensburg, November 5, 1881.

A LUNAR ILLUSION.

[23]—With reference to geometrical illusions, I am sure that many persons must have noticed the following, and yet my attention has never been drawn thereto either in conversation or in print:—The crescent moon being Diana's bow, we shall give the name "sagitta" to the line drawn from the middle of the invisible bow-string connecting the two horns of the moon to the middle of the convex side of the crescent, or the illuminated limb of the moon; and we shall still apply the name, even when the moon is no longer a crescent. The sagitta is necessarily aimed directly at the sun; and yet, to the eye, it always seems to point above, often very considerably above, the sun. The reason is that we are accustomed in drawing, and looking at, pictures on flat surfaces, to consider and treat points or forms as they are projected on the "plane of vision"; but they are really presented to us as projected on a spherical surface, whose centre is the eye. The difference is trifling when the angular field of view is small, and from habit we neglect it, and we are led to do this even when the difference is very important, and we know it to be so. Take the simplest case of the illusion mentioned. The sun is on the horizon, and the moon is 90° distant, not perceptibly different from half-moon; we shall suppose her altitude to be 25°. The moon is at the highest point of the great circle of the sphere of vision passing through the sun, the moon, and the point of the horizon opposite to the sun. As the sagitta points directly to the sun, it lies on that great circle; but as it is at the highest part of that great circle, it is horizontal. Now, when we produce the horizontal sagitta sunwards by the eye, we cannot help picturing to ourselves a line which remains always parallel to the horizon, and our mental production of the sagitta, instead of hitting the setting sun, passes 25° above it.

The illusion is connected with the fact that, in turning to look from the moon to the sun, the observer rotates his head, or perhaps his whole body, round a vertical axis, and not about an axis perpendicular to the plane containing his eye, the moon, and the sun. By turning himself properly in the latter manner, the observer can, by the eye, correctly produce the sagitta so as to hit the sun; it will be better, though not necessary, to screen off the horizon. We have neglected refraction, the effect of which is small comparatively, but in the case considered, the difference of refraction goes to diminish the illusion, and not to help it. M.

[Five or six years ago I put the question raised by "M" before the readers of the *English Mechanic*. It is connected with a question of some interest to artists, viz., the true rules of perspective for pictures including a very large visual area.—ED.]

LATIN QUOTATIONS.—OPTICAL ILLUSION.

[24]—I welcome with much pleasure the appearance of your new scientific journal, and feel sure that it will meet with the support it deserves. It is not everyone who cares to pay sixpence for *Nature*, and the cheaper ones are sadly wanting in the tone and character which I think should pervade everything connected with science. I should like, however, to make one deprecatory remark. I think that the too frequent use of Latin quotations should be avoided in a paper addressed principally to those who have not had an university education. Many who take a lively interest in science have not had time to study the dead languages,* and to them (unless accompanied by a translation) these quotations are simply repellent.

I enclose an instance of an optical illusion just observed. It will be noticed that Mr. Smith's address appears to decline to the left.—Yours truly,

Nov. 9, 1881. C. J. WATSON.

Mr. J. J. SMITH,
Blank Green,
SMITHBOROUGH,
Near BLANKTOWN.

* [There are quotations so familiar that they can scarcely be regarded as belonging to a dead language. Such was the one I used, *Nescit vox missa reverti*, which means that "what has been said cannot be unsaid."—ED.]

OPTICAL ILLUSIONS.

[25]—The No. 1 illusion I made an independent discovery of many years ago, as also, no doubt, have many more who have been much used to drawing geometric and mechanical patterns. Since the advent of KNOWLEDGE, however, I have thought a little more over it. The explanation of this figure is, I believe, to be found in the peculiar movements of the muscles of the eyeballs. The principal movements of the eyeball are vertical and horizontal. Vertical lines are followed by means of one pair of muscles, which move the eyeball through a vertical plane. Horizontal lines are followed by corresponding muscles. Oblique lines are followed by the eye through the combined action of both these pairs of muscles. It is true that there is also an oblique pair of muscles, but from their position, I do not think they assist the eye to follow an oblique line, being more suited to rotate the eye upon the axis of its lenses. Referring now to diagram No. 1, the eye follows the line EG by a simple movement, but in following the line AB, both pairs of muscles are employed. This movement being more complex, is not so certainly continued when the guide of the line is lost by the interposition of the space EH. The eye is led away, as it were, also by the commencement at the point B of the line BG, which breaks the continuity of the oblique movement. It seems to me further complicated by the movement of

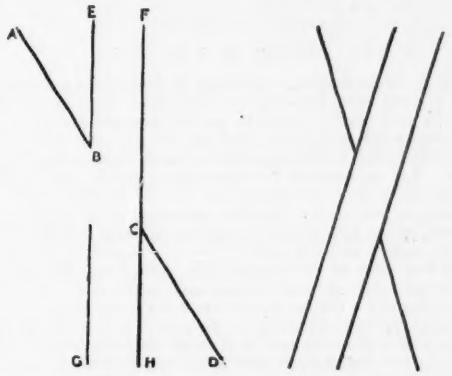


Fig. *a*. Fig. *b*.

two eyes, which work more easily together in the vertical and horizontal movements than in the oblique. This may be noticed when closing one eye, the illusion being then not so complete.

In confirmation of my remarks, I send you two diagrams. If the line EG be broken at B, the illusion is almost destroyed, the line having lost its power of attracting the eye to a simpler movement (Fig. *a*).

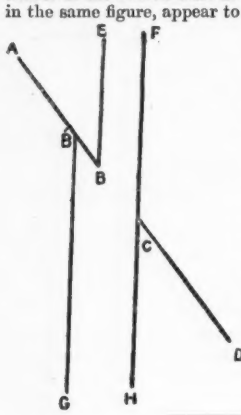
Again, if the same diagram be set before the eye symmetrically disposed to the vertical position, so as to present to the eye similarly situated oblique lines, the illusion will vanish, as then the oblique movements of the eyes are required to follow both the parallel line *and* the one which is opposed to them. This, then, brings the diagram to the form of an ordinary Roman X (Fig. *b*) in which there is no difficulty in following the single line. If this same diagram be set before the eye with the parallel lines either vertically or horizontally disposed, the illusion will reappear. Hoping KNOWLEDGE will be as successful as it deserves, I remain, yours, &c.

W. D. RICHMOND.

XUM

[26]—In his interesting article on "Illusions" in your first number, Mr. Foster writes:—

"The lines A, B, C, D of Fig. 5 appear to be curved so as to be nearer at the middle than at either end; while the lines E, F, G, H, in the same figure, appear to be so curved as to be farther apart in the middle than at either end."



The impression on my mind on looking at the figure is *exactly the reverse* of this. Is the letterpress at fault,* or does the difference depend upon my mental reception of the phenomena?

Referring to Fig. 1, might I be allowed to suggest as an explanation of the illusion, that the mind unconsciously grasps the impression of the vertical distance between EG and FH to the exclusion or depreciation of the greater distance between B and C. I find that if I construct the figure as below, making the distance of B'G from FH equal to the diagonal distance BC, the illusion disappears.—Yours faithfully,

WM. H. ALLEN.

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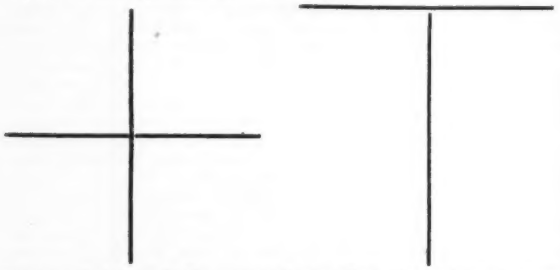
[27]—A correspondent sends the following:—



How much longer is the perpendicular than the horizontal cross-bar?

A NEW THEORY OF THE TIDES.

[28]—Your correspondent, "A Fellow of the Royal Astronomical Society," in your first Number, was very hard upon paradoxers. Would he be very much shocked to hear—if you will permit me to utter the contumacious opinion—that one of the most blundering paradoxes with which I am acquainted is the Newtonian doctrine of the tides? Let us examine this question carefully and briefly. Every one knows that the tide rises or falls simultaneously on opposite sides of the earth. Newton endeavoured to explain this phenomenon by the hypothesis that the moon attracted the water on the side nearest to itself, and at the same time drew the earth away from the water on the opposite side. This theory has been accepted with universal approval, but can anything be more erroneous, not to say absurd? We are coolly asked to believe that when the sun and moon are in conjunction on one side of the earth, drawing up the tide, one of the most mobile of substances, the water, on the other side, is left bulging out into space, where there is no rival attraction to hold it, and that it is capable of resisting three attractions—that of the sun, of the moon, and of its own earth—all at once!

If this is the best explanation that can be given of the tides, no wonder that Aristotle proclaimed the difficulty to be "the grave of human curiosity."

I maintain that it is a monstrous scientific fallacy.

Those who pull down ought to be compelled to reconstruct; those who denounce one system are bound to offer some substitute which may be considered preferable. Therefore, I must not shrink from this imperative task. With your permission I will here give a quotation from my paper on "The Astronomy of the Future" in "Pith":—

"What we say is, that the magnetic attraction of the moon, as a magic wand or beam, penetrates the earth from one side to the other—sword-like it sheathes itself in the diameter of our globe. At the spots where this spiritual and invisible falchion makes its entrance and exit, the waters rise to meet it in obedience to its mighty beckoning and summons; and as the moon is for ever shifting its position, so the waters are for ever chasing over the surface of the globe the two mundane extremities of the moon's irresistible, triumphant electrical wand! Or we may, as an alternative, assume that this magnetic influence, which is attractive on one side of the earth, becomes repellant on the other."

I now wish to invite your readers' attention to another point connected with the Newtonian System of Astronomy. I fancy that we must discard the theory of "universal gravitation" which has been so belauded, in favour of the theory which I have termed "The Polarity of the Universe." Gravitation, with its clumsy addition of a centrifugal force, is simply an impossible explanation of the movements of the heavenly bodies. Centrifugal force, which is entirely of a mechanical origin, cannot be originated and sustained between two bodies unless they are physically connected and moved from a central power; whereas magnetic polarity, with its attraction and repulsion, is a satisfactory solution of the phenomena of motion in the solar system. As long as a planet presents its poles in a slanting direction towards the sun, the revolution of the planet is an everlasting dynamic certainty, and it is quite possible that the sun may possess a number of poles, one for each planet.

In the first article on "Comets," you say that Newton discovered that their motion was regulated by the law of gravitation. I contend that this discovery was a delusion. We know that some comets move off in curves, which render their return to our system an impossibility; and if there occurs, in Nature, one exception to a law, what becomes of such law? It is null and void. I submit that comets *do* obey the laws of polarity, and that they *do not* obey the law of gravitation.

I believe that we must abandon the teaching of Sir Isaac Newton, and consider him one of the paradoxers and scientific old fogies of the past.—Yours, &c.,

NEWTON CROSLAND.

London, *Nov.* 12.

[I fear Mr. Newton Crosland belongs to the class of confirmed paradoxers, those who having encountered difficulties which they have been unable to surmount, suppose that they have made discoveries concealed from others. As a mere matter of fact, the accepted theory of tides would give (apart from effects of oceanic friction) low water under the moon and opposite that region; yet the Newtonian theory that the moon's attraction would of itself cause a leaping up of the water, both under the moon and opposite, is sound. Gravitation requires no centrifugal force, and, indeed, it is only in treatises by ill-informed writers that the theory of a centrifugal force as such is propounded. What is called centrifugal force is in reality simply the measure of what the centripetal force does, it is only another way of viewing the centripetal force. I believe Mr. Crosland's views will be received when Newton is entirely forgotten—but not till then.—ED.]

DR. FERRIER AND VIVISECTION.

[29]—A distinguished professor at one of our colleges once said to me: "The manner in which our countrymen receive the conjectures of popular teachers of new doctrines only proves what most of us knew before, that they are the most gullible of all intelligent nations." The remark may be applied not only to the votaries of science, but to the followers of any agitator who from the rostrum calls upon his supporters to uphold or denounce what he considers to be right or wrong. As a nation, we are opposed to cruelty and oppression. We boasted in former days of our Anti-slavery Leagues, and our Society for the Prevention of Cruelty to Animals receives (and justly) a large share of public support. If a man kicks his donkey or starves his dog, he is speedily brought before a justice of the peace, who rarely fails to punish the offender. Last Thursday the humanitarians applied for a summons against Dr. Ferrier, under the Vivisection Act, for performing an experiment which was described as being "frightful and shocking."

The summons was granted, and long ere this many thousands of kind-hearted people of both sexes have given vent to their horror of the professor's experiments on the two unfortunate monkeys, and doubtless have settled in their minds that he is a fit subject to be made an example of, and worthy of punishment.

The vexed question of vivisection has been well ventilated by abler pens than mine. It is not my intention to say a word either for or against the practice, but the impartial reader should pause before he passes sentence in this matter. *Audi alteram partem* should be the motto of every right-thinking Englishman, and important subjects, particularly those relating to medical science, should be carefully analysed before an opinion is expressed, and such a man as Dr. Ferrier ought not to be placed in the category with the human brutes who daily appear in the dock to answer charges of wanton cruelty.

[* The error is in the figures; the upper should have been the lower.—ED.]

I have long watched with interest the results of Professor Ferrier's labours in the cause of humanity, and a few words upon the nature of his experiments may not be out of place here.

Physiology is, comparatively speaking, a new science. Before the discovery of the circulation of the blood, physicians were little better than charlatans. Many of the early philosophers were mere dreamers; they made little or no advancement in the natural sciences, but their extravagant ideas influenced civilised nations for nearly twenty centuries. Aristotelianism and peripatetic dogmas *hindered* the progress of science. When the great Bacon arose giant-like, amidst the chaos of hypotheses and fruitless doctrine, and laid the foundations of the inductive and experimental philosophy, then a new era commenced, and scientific progress followed.

There is no subject in physiology more attractive than the study of the human brain; that wonderful and complex organ has occupied the attention of philosophers in all ages, but little light has been thrown upon its functions until recently. Several physiologists of the seventeenth, eighteenth, and early part of the present century, wasted a deal of their time in fruitless searches after the exact seat of the soul. Some held that the soul was equally diffused through every part of the body. Descartes maintained that it is in the pineal gland of the brain, and Borri asserted that in the brain is formed a certain very subtle, fragrant juice, which is the principal seat or residence of the soul, and that the subtlety and fineness of the soul depended on the temperature of this liquor, rather than on the structure of the brain. Now, all these fanciful theories were very interesting to read, but they did not benefit mankind, and it was not until Flourens commenced his experiments upon the brains of animals that any definite knowledge of the functions of the cerebral organ was arrived at.

A worthy follower in the footsteps of the French physiologist is Dr. David Ferrier. The work of the latter on the "Functions of the Brain" has been justly described as "marking the end of an old era and the beginning of a new one in cerebral physiology." In briefly summing up the results of Dr. Ferrier's experiments on the brains of animals, I wish to direct attention to the localisation of two or three of the cerebral functions as demonstrated by the learned professor. The upper end of the Fissure of Sylvius, called the angular gyrus, is the centre of the sense of sight. When that part of the cerebral substance is destroyed, the sense of vision is gone in the opposite eye, though all the other senses remain, and an animal can hear, and smell, and taste, and touch, and perform every voluntary action as before. "It is a remarkable fact," says Dr. Ferrier—and let it comfort you, oh ye anti-vivisectionists—"that the brain is unsusceptible to every kind of irritation except electricity. You may cut and cauterise the brain without exciting sensation, although it is the organ of feeling. This we have learnt from experiments and the testimony of men who have had their brains injured." In another convolution of the brain, called the temporo-sphenoidal lobe, Dr. Ferrier found the centre of the sense of hearing—from the lower extremity of the lobe proceeds a large tract or process which is called the olfactory tract. When this part of the brain is destroyed, the sense of smell is abolished. All these discoveries explain certain facts which have been observed in connection with disease. Of course, I have only selected a few of the many illustrations of the Professor's experiments, and it only remains for me to point out the vital importance of such discoveries to the medical profession, and, I may add, to all who are suffering, or likely to suffer, from cerebral disease. It is impossible to experiment upon the human subject. There is no alternative but to practise upon the living animal, if an accurate knowledge of the functions of that important organ is to be gained.

Surely, when the facts are known, few will accuse the Doctor of wanton cruelty. All his experiments are made for the benefit of suffering humanity and the advancement of science. To conclude, in the Professor's own words:—"You may rest assured of this, that every addition to our knowledge of the brain will inevitably lead to a better appreciation, and more successful prevention and treatment of a large, and it is to be feared, rapidly-increasing class of distressing diseases of the brain and nervous system, which, even to those best acquainted with them, are still involved in profound obscurity."

W. L.

A. PLANET'S MOVEMENTS.—THE TIDES.

[30]—In looking over the first number of KNOWLEDGE, I find on page 13 the following—for an explanation of which I shall feel thankful.

1. "The planet Mars, it will be noticed, passes during this month what is called a stationary point; he is advancing (moving from right to left) till November 17th, after which he retrogrades. But he does not really come to apparent rest, owing to the wide sweep of the loop he forms between his stationary points. Jupiter, Saturn, and Neptune are all three retrograding." 1. Do the planets move in their orbits uniformly, or turn backwards and forwards, or how? 2. What is the cause for this, and is this retrograde motion performed during each revolution; if so, how often? 3. What am I to understand by "Mars not coming to apparent rest, owing to the wide sweep of the loop he forms between the stationary points?" What does this loop signify; and is there really such an occurrence as Mars or the other planets coming to a rest, &c.?

2. In Guillemin's "Heavens," page 391, in note by R. A. I., I find the following:—"If we consider only the dynamical relations, we find that the place of low-water should be under the moon, and at the opposite part of the earth, the place of high-water between these regions." Will you kindly explain how the point of high-water ought not to be directly under the moon, but at a point at right angles to it? ZETA.

[The movements referred to are only the apparent motions of the planet in the heavens. The planets constantly advance in their own orbits. I will take a later opportunity to answer "Zeta" more fully on both subjects.—ED.]

WHAT IS THE CAUSE OF GRAVITY?

[31]—I was glad to see the appearance of a paper like this, giving the views of the best men of to-day about the more interesting things in nature in plain language. As you invite correspondence, permit me to state that, although a toiler who cannot find much time for reading, I take, nevertheless, a very great interest in the study of nature. It gives me hope and affords me pleasure. And, as you say, it is certainly religious and improves our feelings. As regards myself, I take an especial interest in the law of gravity, not as to how, but as to its cause. I have read that Sir Isaac Newton could not tell. I should feel very grateful if you would give us the various theories that have been given out on this subject, no matter how silly they be, in as short a form as possible. On inquiry, the answer is always "Attraction"—a meaningless word. I have for many years observed every phenomenon resembling the action of gravity, and have come to conclusions as to what its cause is, that on careful comparison harmonise with all the well-known and undoubted fundamental laws of nature. I have read of experiments made and theories submitted that in no way come near the mark, but I may not have reached the right source yet. Experiments to prove gravity are difficult to make because we cannot exclude the earth's gravity from them even for a moment. But, by the help of reason, it can be made, and even clear to a mind that can comprehend other natural phenomena that can only be seen by the mind's eye. But, as soon as the idea has been grasped, numberless things come to view that resemble the earth's gravity on a smaller scale. "Attraction," as a word, is misleading, for to attract there must be a substance having these impossible qualities of pulling down every kind of matter in a vacuum of any substance.

However, to write my view on this would be months of labour and certain ruin. I merely wanted to suggest how interesting a subject this would be to many more.

Yours, &c., F. J. D., "SELBST."

[The subject is, doubtless, most interesting. It is, also, exceedingly difficult. Does "Selbst's" theory accord with the demonstrated fact that the action of gravity is communicated far more rapidly than light travels?—ED.]

PHRENOLOGY.—VECTORS.—INTONING IN SYNAGOGUES.

[32]—I don't know whether you will consider the following questions worth asking or answering in KNOWLEDGE.—

1. Assuming that phrenology is all wrong, *what are the causes* that determine the shape of the head? I am myself a sceptic, in the proper sense of the word: but Lewes' opinion, that the brain acts as a whole, and that its functions are not localised, seems to me, *à priori*, highly improbable. 2. Having lately begun to take great interest in mathematical physics, I read carefully Maxwell's little book on "Matter and Motion." The mode of treatment was almost entirely strange to me; and what I want to know is, whether the new method has any special value: whether any, or what peculiar benefits are gained by the use of vectors in mechanics, as distinguished from the old geometrical and analytical methods. In any case, Maxwell's treatise wants a good deal of expansion and illustration to make it intelligible to a tyro. 3. My last question, I am afraid, hardly falls within your province, unless you take in *omne scibile*; but it is one to which I have hitherto failed to get an answer. In reading the Scriptures in the Jewish synagogue, a certain intonation is employed. Is this expressed or expressible in the ordinary musical notation, or in any musical notation whatever, and if so, where could it be found? G. P.

XUM

Queries.

[11]—Logarithms.—Will you kindly state the best cheap "Table of Logarithms and Antilogarithms," &c., suitable for a general science student, but more especially for one preparing for examination in "mine surveying," in connection with the City and Guilds of London? He would observe that he has seen a book of tables advertised at 2s., and written by W. C. Unwin, and if you think that will be quite suitable, "yes" opposite the initials will be deemed a sufficient answer. The writer was intending to purchase the above book, but on reading your remarks on logarithms in No. 1 of Knowledge, determined to seek your advice.

James Grundy.

[We use ourselves Chambers's "Tables" for ordinary work, and Hutton's when certain subsidiary tables are required. We have not used the work named above, but probably some of our readers can give Mr. Grundy the information he requires.—Ed.]

[12]—Is it possible for the same star to be both morning and evening star on the same day? Does not Tennyson's Hesper—Phosphor refer to the same star at different periods of its month?—R. A. Bullen. [To the first question, No; to the second, Yes, only for month should be written synodical period, meaning the period in which the planet goes through all its apparent movements with respect to the sun, from being in conjunction with him to its next return to that position.—Ed.]

[13]—Water Spheroids.—If a little water is spilled from a height of a few feet upon a surface of smooth water, a portion of the fluid, instead of mixing with the rest at once, is formed into small globes or spheroids which, I think, seldom if ever exceed ¼ inch in diameter, most of them being very much smaller. These little drops move over the water for distances varying from a few inches to a few feet, and then gradually though rapidly diminish and disappear.

In explanation of this very common phenomenon, I have been informed that a layer of air of greater density than the atmosphere is formed over the surface of the water, and that the part of this layer between the globules and the plain surface of water offers so much resistance that for a time the two bodies of fluid are unable to unite. It would be interesting to know what causes this denser layer of air, and why it is only for a time able to overcome the force of gravity acting upon the spheroids.—E. C. R.

[14]—Velocity of Sound.—I understand Tyndall to say that the increase of temperature in the condensed part of a sonorous wave augments its velocity one-sixth. Is the increase gained by the rise of temperature in the condensed portion not counterbalanced by the decrease of temperature in the rarefied part?—Sound.

[15]—The Missing Link.—What is the "missing link," of which we hear so much in connection with Mr. Darwin's books? Is it what should come between an ape and a man? An answer on this point would much oblige—An Ignoramus.

[16]—German and English.—Is there any work where I can find an account of the progress by which the English and German languages have come to differ so widely in character from each other? In good English writing we find the arrangement of subject, predicate, &c., quite different from that adopted in good German writing. Yet the languages had a cognate origin, and must once have resembled each other somewhat closely in those points in which they now differ. Strangely, too, while the German language has become in structure less simple than the English, the German words for abstract ideas, and their scientific terms, are much simpler than ours. When we speak of oxygen, a German speaks of sour-stuff; but where an English writer would say that a mixture of oxygen and hydrogen will explode, under such and such conditions, a German would say that sour-stuff and wasser-stuff, in certain proportions mixed, and under certain conditions so-and-so treated, explode shall.—Eclecticus.

[17]—Retrogradation of a Planet.—Which planet has the longest arc of retrogradation? And on what circumstances does the length of the arc depend?—Astronomicus.

[18]—Chemical Treatises.—Can any reader of Knowledge tell me what books will give me the best general idea of the so-called new chemistry?

Arctic Navigation.—Advices from Copenhagen state that the news received from the Dutch Polar Expedition on board the schooner *William Barents* is very unfavourable. Owing to the continuous ice barrier which extends nearly to Norway, Spitzbergen could not be reached, nor even the Bear Islands; and after one more attempt to force through northward, the expedition will return home, as the captain is convinced that this year Nova Zembla is completely enclosed in a barrier of ice.

Replies to Queries.

[1]—Ultima Thule.—The expression "Ultima Thule" occurs in Seneca's "Medea," Act. iii., verse 375:—

> —— Venient annis
> Sæcula series, quibus oceanus
> Vincula rerum laxet, et ingens
> Pateat tellus, Typhisque novos
> Detegat orbes; nec sit terris
> Ultima Thule.

The passage has been quoted to show that the ancients had heard of the New World, as our school books call the ancient continent of America. It seems, however, to prove rather the reverse, as showing that Thule (whether Thule were Iceland or one of the Orkneys, Shetlands, or Hebrides) was, in Seneca's time, regarded as the remotest-known region of the earth.—X. Z. Z.

[1]—Ultima Thule.—Pytheas, a citizen of Massilia (Marseilles), previous to the time of Alexander the Great, undertook a voyage of discovery to the far north. The regions discovered by him were enveloped in fog and "chaos" and uninhabited. He designated the farthermost limit of his discovery Thule, whence "Ultima Thule." The actual identity of Thule is shrouded in mystery. Of Jutland, Shetland, Norway, and Iceland, each claims precedence as the Thule of Pytheas. Speaking offhand, I believe Pliny uses the expression "Ultima Thule." I cannot recollect the way in which he used the term. Some imagine Thule as a creation of the poets to express the extreme limit of the world.—W. G. Rolfe.

[1]—Ultima Thule.—If your correspondent is a classical scholar he may refer to Ptolemy, Tacitus, and others. Pliny describes it as "an island in the northern ocean discovered by Pytheas after sailing six days from the Orcades." I quote the following from Brewer:—"Called by Drayton, Thuly. Pliny, Solinus, and Mela take it for Iceland; others, like Camden, consider it to be Shetland, still called Thyleus-el (Isle of Thylë) by seamen, in which opinion they agree with Marinus and the description of Ptolemy and Tacitus." Bochart says it is a Syrian word, and that the Phœnician merchants who traded to the group called it Gezirat Thulë (Isle of Darkness); but probably it is the Gothic Thule, meaning the "most remote land," and is connected with the Greek telos, the end.

> Where the northern ocean in vast whirls,
> Boils round the naked, melancholy isles of farthest Thulë.
>
> Thomson, "Autumn."

Thule was the most northern part known to the ancient Romans.

> Tibi serviat ultima Thule
>
> Virgil, "Georgics" i., 30.

—R. T. Wright.

[3]—Flight of Birds.—If "Aërial" considers for a moment the very small proportion the bones of a bird bear to its bulk, and the still smaller cavities he speaks of, I think he will see that the heated air the latter contains can but very slightly assist the bird's flight, or rather buoyancy; besides which, the heat of a bird would only cause a slight expansion of the enclosed air. Is "Aërial" sure the bone cavities do contain air, and not some gas?—E. C. R.

[3]—Flight of Birds.—The flight of birds cannot be aided in any appreciable degree by the presence in the air-passages of the bones of air, lighter (because warmer) than the air in which the bird is flying. The actual supporting power obtained in this way can be easily shown to be utterly insignificant. It is simply the difference between the weight of the air in the passages and that of an equal volume of the outer air. This is less, of course, than the weight of a volume of air equal to the volume of the air-passages, and much less than the weight of a portion of air equal in volume to the bird, which would correspond to a very small fraction of the bird's actual weight, perhaps about one 500th part. The support obtained in this way cannot be at the utmost more than the 10,000th part of the bird's weight, and is not worth considering in dealing with the question of flight.—Fitful.

[4]—The Earth's Inclination.—In reply to "Moonstruck's" query, I should say that if the earth's axis was perpendicular to the plane of the Ecliptic, the weather at the Poles would be much colder than a Polar summer at present, because the rays of heat would fall very obliquely upon the earth surrounding the Poles. At the Equator the heat would be greater than at present, because the sun would daily appear to cross the zenith.—E. C. R.

[4]—The Earth's Inclination.—If the earth rotated on an upright axis, there would be perpetual spring or autumn all over the earth; but it must be remembered that spring and autumn at the equator are the hottest seasons of all, while at the poles they mean a sun just skirting the horizon, so that the coldest winter in temperate regions would not be so cold as this so-called polar

spring. To put the matter more definitely. If the earth rotated on an upright axis, the sun would everywhere, all the year round, rise exactly in the east (apart from the slight effects of atmospheric refraction) and set exactly in the west, attaining at midday an elevation equal to the complement of the latitude—90° at the equator (which would bring the sun overhead, 70° in latitude 20; 40° in latitude 50; 20° in latitude 70; and having no elevation at all in latitude 90, or at either pole.—STUDENT.

[8]—VOLUME OF SPHERE.—The simplest way of showing that the volume of a sphere is two-thirds that of the enclosing cylinder, is given in the following sketch, which, if "Archimedean" is anything of a geometrician, he will have no difficulty in filling in:—Show, first, that the surface of the sphere is equal to the *curved* surface of the enclosing cylinder, by taking any two planes close to each other through sphere and cylinder, parallel to the top and bottom of the latter, and showing that the part of spherical surface between these planes is equal to the part of cylindrical surface between them. This having been shown, imagine the surface of sphere, thus known, divided into indefinitely small areas, each of which may be regarded as the base of a pyramid, having the centre of the sphere as vertex—all these pyramids together giving the volume of sphere. Their combined volume is equal to that of a pyramid having a base equal in surface to the surface of the sphere (that is, of its enclosing cylinder, by what has been already shown), and a height equal to the radius of the sphere. This volume, by well-known property of pyramid, is represented by one-third the product of numbers representing the curved surface of cylinder and the radius of sphere. But curved surface of cylinder

= circumference of base × 2 radius of sphere

= circumference of a great circle × 2 radius of sphere.

Hence volume of sphere

$$= \frac{1}{3} \text{ radius} \times \text{circumference of a great circle} \times 2 \text{ radius}$$

$$= \frac{2}{3} (\text{radius})^2 \times 2\pi (\text{radius}) = \frac{4\pi r^3}{3} \text{ (if radius} = r).$$

But volume of enclosing cylinder = area of base × 2 height

$= \pi r^2 \times 2r = 2\pi r^3$.

Hence volume of sphere is two-thirds that of cylinder.

MATHEMATICUS.

EFFECTS OF LIGHTNING ON TREES NEAR A TELEGRAPH WIRE.—Some instructive facts in this connection have been brought to light by M. Montigny, in recent examination of poplars bordering part of a road in Belgium, between Rochefort and Dinant. The part in question is some 4,600 mètres in length, and runs westward; it is level for some distance, then rises gradually to a height of 61 mètres, through a wood, traverses a wooded plateau 200 mètres in extent, then descends, still through wood, to a plain. A telegraph wire runs near the row of Virginia poplars on the north side, and it appears that, out of nearly 500 poplars forming this row, 81, or a sixth, have been struck by lightning. Hardly any have been struck in the other row. The trunks have been mostly struck on their south side, and nearly opposite the wire. Comparing different portions of the road, it is found that in the horizontal part none of the (129) trees show injury from lightning, or at most only one (a doubtful case), but as the road rises through the wood, the cases quickly multiply, and on the wooded plateau as many as nine out of 14 trees, or 64 per cent., have been struck. On the slopes the proportion is 25 per cent. M. Montigny distinguishes three kinds of injuries—(1) the bark torn and detached on a limited part of the trunk; (2) a furrow, straight or (rarely) spiral, made on the tree, from near the wire, down to the ground; and (3) a peculiar oval wound, with longer axis vertical, and lips coloured light brown. Now, the furrows, which are probably due to the most violent discharges, are relatively most frequent on the plateau and on the western slope, which the storms usually reach first. M. Montigny is of opinion that the lightning, while attracted by the wire, does not strike this first, then the tree, but strikes the tree directly. His conception of the process is to the following effect:—Suppose a thunder-cloud charged with positive electricity. A long telegraph wire under it, though insulated, may acquire as great negative tension in the nearest part as if in direct communication with the ground, and the tension is greater the nearer to the cloud. While the inductive influence affects the wire most, near objects, such as trees, share in the influence according to their conducting power. The lightning, attracted in the direction of the wire, yet does not strike this, the insulating cups presenting an obstacle to its prompt and rapid escape. It finds a better conductor to earth in a neighbouring poplar, wet with rain. From the facts indicated it results, that of two similar houses, one built on a plain, the other in a wood, and having a telegraph wire fixed to them, the latter is the more liable to injury by lightning, and the danger is greater if the wood enclosing the house be upon an eminence.—*Times.*

Our Mathematical Column.

FROM the way in which logarithms are commonly spoken of, one would suppose that they were originally intended to perplex the student, instead of having been devised specially to assist him. It may be that this is due chiefly to the use of a name whose real meaning is not known, while, were its real meaning known, the use of numbers so named would still remain a mystery to all save mathematicians. The word logarithm is really intended to signify ratio-number. But hundreds who use logarithms, and thousands who would do well to use them, would not be one whit the wiser for knowing that there are tables of numbers which may be regarded as ratio-numbers. If a name had been given to logarithms which suggested to all something of their real use to computers, we should find them more valued and more commonly employed than they are. But unfortunately the singular idea that nothing but a long unintelligible name is worth anything in science—an idea about as worthy of respect as the Barbadoes mother's admiration for the name Chrononhotonthologos which Captain Marryat gave to her baby—has caused these useful tables to bear a ridiculous because un-English name.

In reality, a logarithm *is* a ratio number, though that says very little to most men of its use. The logarithms of the numbers 1, 2, 3, 4, 5, &c., up to 100,000 in our books, are in reality the powers to which 10 must be raised to give the numbers 1, 2, 3, 4, 5, &c., respectively. If we know the power to which 10 must be raised to give the number 13, and also the power to which 10 must be raised to give the number 17, we have only to add these powers together to obtain the power to which 10 must be raised to give the product of 13 by 17, or 221. If our tables give all the powers of numbers from 1 to 221, we need not actually multiply together 13 and 17 to get their product 221. All we need do is to add the logarithm of 13 to the logarithm of 17; the sum is the logarithm of 13 and 17 multiplied together. We look out in our tables the logarithm corresponding to the sum of the logarithms of 13 and 17, and we find that the number corresponding to this logarithm is 221.

Here, of course, we have not been saved a particle of labour. While we were looking out these logarithms, a charity boy could have multiplied 13 and 17 together half a-dozen times.

But, supposing we had occasion to multiply together the numbers 21,714, and 56,912, and to divide the product by 78,124 and 62,315; that is, to divide the product of the first pair of numbers by the product of the second pair of numbers. Then, if we know the powers to which 10 must be raised to give the above four numbers respectively, we can tell, by simple addition and subtraction, the power to which 10 must be raised to give the answer to our little sum; and if we know also what is the number which would result from raising 10 to the power just mentioned, this number is the answer we require. Suppose, for example, that $10^a = 21{,}714$; $10^b = 56{,}912$; $10^c = 78{,}124$; $10^d = 62{,}315$. Then we know by the properties of ratios that

$$21714 \times 56912 = 10^{a+b}$$

$$78124 \times 62315 = 10^{c+d}$$

while

$$\frac{21714 \times 56912}{78124 \times 62315} = 10^{a+b-c-d}$$

Now a table of logarithms gives us for all numbers from 1 to 100,000 the powers corresponding to a, b, c, and d in the above example. (We can find such powers also easily enough from the tables for all numbers from 100,000 up to 10,000,000.) So that all the computer in the above case would have to do would be to look out the powers corresponding to a, b, c, and d, to add a to b and c to d, subtracting then the latter sum from the former. The result would be the power to which ten must be raised to give the answer to his sum; and he would only require to find that power in his tables to get the number he wanted. He would, probably, do all this in about the time that a first-rate computor would have got half way through the multiplying of 21,714 and 56,912 together.

This is, of course, only a general account of the use of a book of such powers, or of a book of logarithms. It should suffice to show that, despite their hard name, they are worth understanding by all who have much computing to do; and there are few who would not do well, by studying a little the use of logarithms, to make themselves ready to employ the tables when occasion may arise.

Turn now to a few details.

The logarithms given in the tables are in reality the logarithms of the numbers 1, 2, 3, 4, 5, 6, 7, 8, 9; 1, 1·1, 1·2, 1·3, 1·4, 1·5, 1·6, 1·7, 1·8, 1·9; 2, 2·1, &c.; that is, however large the number in the lefthand column, it is understood to lie between 1 and 10, so that its logarithm lies in value between 0 and 1 (ten to the power *nought* gives 1, and ten to the power 1 is ten). Decimal points are not

XUM

shown in the tables, but in reality they are implied. Thus, the tables give—

the logarithm of 3191 as 5039268;

this really means that—

the logarithm of 3·191 is 0·5039268. (A.)

(The student should here turn to his tables and see for himself how the matter stands; he should also ask himself what statement A. really means. On one side we have the number 3·191; on the other, a decimal fraction slightly exceeding ½, which we are told is the logarithm of 3·191. Now, remember that the logarithm of a number is that power to which 10 must be raised to give the number, and we see at once how A is to be interpreted. For 10 to power ½ is the same as the square root of 10, which we know to be rather greater than 3, so that in the equation

3 + some small fraction = $\sqrt{10}$ or $10^{\frac{1}{2}}$

we have a rough approach, near enough to illustrate its meaning to the statement—

the logarithm of 3·191 = 0·5039268.

A few inquiries of this sort, with a book of logarithms in hand, will very soon give the intelligent reader a good idea of their meaning and use. Take one other case. We find from the tables that

the logarithm of 2 is ·3010300.

This means that 10 raised to the power $\frac{30103}{100000}$ is equal (very approximately) to 2. Now let us see whether taking the simple fraction $\frac{3}{10}$ we find 10 to the power $\frac{3}{10}$ approach in value to 2, though, of course, not quite so closely as would 10 to the power 3010300. We know that $10^{\frac{3}{10}}$ is the same as the 10th root of 10^3 or of 1000; and we know that this is not very far from 2, for 2 raised to the tenth power gives 1024.)

But the tables are not the less complete that they only give the logarithms of such numbers as 3·191, 9·874156, and so forth. For, since shifting a decimal point one place to the right means multiplying by 10, while shifting the point one place to the left means dividing by 10, and since the logarithm of 10 is 1, we have only to add or subtract 1, 2, 3, 4, 5, &c., to the logarithm given in the table, to get the true logarithm of any number whatever within the limits ranged over by the table. Thus—

Logarithm of 3·191 = 0·5039268

Logarithm of 31·91, or of 10 × 3·191 = log. 10 × log. 3·191 = 1·5039268

Logarithm of 319·1, or of 10^2 × 3·191 = 2·5039268

Logarithm of 3191, or of 10^3 × 3·191 = 3·5039268

and so forth, the whole number in the logarithm being always 1 less than the number of digits in the integral part of the original number (that is, of digits on the left of the decimal point).

Again, logarithm of 0·3191, or of 3·191 ÷ 10 = 0·5039268 − 1, which for convenience and symmetry is written—

Logarithm 0·3191 = $\bar{1}$·5039268

Logarithm of 0·03191, or of 3·191 ÷ 10^2 = $\bar{2}$·5039268

Logarithm of 0·003191, or of 3·191 ÷ 10^3 = $\bar{3}$·5039268

and so forth, the whole number under the minus sign being always 1 more than the number of cyphers following the decimal point of the number.

It will be observed that in the case of numbers less than 1 we do not follow what might, at first sight, seem the obvious course, of subtracting the tabular number from 1, 2, 3, or whatever is the number under the *minus* sign, leaving the logarithm negative. Thus we write 1·5039268 short for −1 + 0·5039268; we do not write −0·4960732. The reason is that it is found convenient to have *all* the quantities on the right of the decimal point positive throughout all computations.

MATHEMATICAL QUESTION.—Is there any solution of either or both the following sets of simultaneous equations?

$x^2+yz=a^2$ (i) $\quad x^2+xy+y^2=a^2$ (i)

$y^2+zx=b^2$ (ii) and $\quad y^2+yz+z^2=b^2$ (ii)

$z^2+xy=c^2$ (iii) $\quad z^2+zx+x^2=c^2$ (iii). A. N. J.

We know of no solution to the former set; that is, it cannot be made, so far as we know, to produce a quadratic equation. The second set may be solved thus ("A. N. J." will probably only need to be shown the steps):—

Subtracting (ii) from (i) gives $(x+y+z)(x-z)=a^2-b^2$ (iv).

Subtracting the sum of the squares of (i) (ii) and (iii) from twice the sum of the products (i) × (ii), (ii) × (iii), and (iii) × (ii), we obtain

$$xy+yz+xz=\sqrt{\tfrac{1}{3}[2a^2b^2+2b^2c^2+2c^2a^2-a^4-b^4-c^4}$$
$$=p^2 \text{ (say) (v)}$$

adding (i) (ii) (iii) and 3 (v) we get

$$x+y+z=\sqrt{\tfrac{1}{3}(a^2+b^2+c^2+3p^2}=m \text{ (say) (vi)}$$

From (iv) and (vi) $m(x-z)=a^2-b^2$,

and similarly, $m(y-x)=b^2-c^2$

whence, subtracting, $m(y+z-2x)=2b^2-a^2-c^2$

$$=m(m-3x)=2b^2-a^2-c^2$$

or $$x=\frac{1}{3m}(m^2+a^2+c^2-2b^2)$$

By symmetry $$y=\frac{1}{3m}(m^2+b^2+a^2-2c^2);\ z=\frac{1}{3m}(m^2+a^2+b^2-2a^2).$$

Our Whist Column.

BY "FIVE OF CLUBS."

WE give this week a simple whist game, showing the inferences which can be drawn from the play by one of the players (the leader), and making notes also on the play as it proceeds. The inferences are all of the simplest kind, supposing the game to be conducted according to the accepted principles for sound play. This will appear as we proceed, in later papers, to develope these principles.

THE HANDS.

Diamonds—Q, 9, 7, 2.
Spades—Q, Kn, 10, 3.
Hearts—Q, 6.
Clubs—Kg, 6, 4.

Diamonds—Kn, 10, 8, 6, 4.
Spades—8, 7, 4.
Hearts—Kg, 2.
Clubs—10, 5, 3.

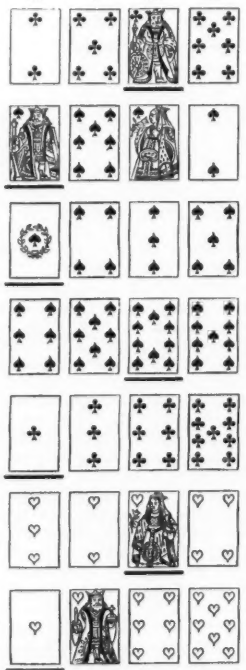
B
Dealer.
Y Z
Trump Card, Diamond King.
A

Diamonds—A, Kg.
Spades—9, 5, 2.
Hearts—Kn, 10, 9, 8, 4.
Clubs—Q, 9, 7.

Diamonds—5, 3.
Spades—A, Kg, 6.
Hearts—A, 7, 5, 3.
Clubs—A, Kn, 8, 2.

Score: — A B = 0; Y Z = 3.

NOTE.—The underlined card wins trick, and card below it leads next.



A'S INFERENCES.

1. Either 3 C and 4 C are both with *B*, or else *Y* or *Z* is signalling for trumps. *B* has not the Queen.

NOTE TO TRICK 1.—Having five trumps, one honour, and his partner having an honour, *Y* would be justified in signalling for trumps were the score low, but not as the score stands.

2. *B* has Kn S, 10 S, and probably one or more small Spades. *Z* is not signalling for trumps, and therefore has neither 3 C nor 4 C.

NOTE TO TRICK 2.—*A* does well to take the trick and return the Ace, thus leaving *B* the command of the suit.

3. *Y* has signalled, and therefore has either 4 C or 3 C; the other being with *B*. As *Z* turned an honour, *Y* and *Z* are probably two by honours, in which case *A B* must make five by tricks to save the game.

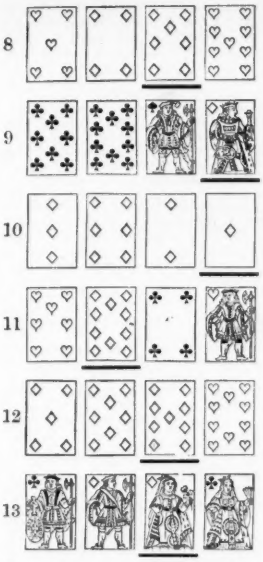
4. The last Spade is with *B* (the Knave).

5. *B* has 4 C, but no more Clubs.

NOTE TO TRICK 5.—*B* returns the highest of two cards.

6. *Z* has not the King of Hearts; *B* has not the Knave (Hearts must be *Z's* best suit, trumps being *Y's*).

NOTE TO TRICK 6.—Under the circumstances *Y* should have played the King. It is his best chance of getting a lead.



7. *Y* has the Knave of Hearts. The only chance of saving the game lies in the probability (*A* having four Hearts, and *Z* a long suit of Hearts) that *B* may be able to ruff Hearts next round.

8. If *Y* has played properly, all his remaining cards should be trumps, and the game is lost.

NOTE TO TRICK 8.—If *Y* trumps at all he should play his highest. His small trump is useless. He should know that the best Heart is with *Z*.

9. *Z* has the Ace of trumps (Kn S being a thirteenth card).

NOTE TO TRICK 9.—*Z*'s play here is bad. He knows his partner has an honour, with a strong hand in trumps, and two tricks only are wanted to win the game, besides the two certain tricks *Z* holds in his hands.

10. If *B* has not Queen and 10 or 9 of trumps, the game is lost, for the remaining cards in *Y*'s hand must be trumps (one honour at least).

11. *Z* should have led the Queen of Clubs. The Knave of Hearts is equally a winning card of its suit; but the play should have shown *Z* that *B* has a small Club, and that therefore *Y* can win trick 11 with his smallest trump. As it is, *Y* has to play highest third hand. *B* does rightly in not over-trumping. If he did, the remaining two tricks would be *Y*'s, but by letting trick 11 pass, *B* remaining with the tenace in trumps (and knowing *Z* to be without trumps), has the two last tricks sure.

Our Chess Column.

SIR,—In your issue of the 11th inst. you gave an admirably-arranged perspective summary of the openings. In reference to the Evans Gambit declined, you say:—Declining the proffered Queen's Knight's pawn subjects the second player to a cramped defensive game. I beg to draw your attention to the fact that recent analytical researches have proved that by declining the Evans Gambit with B to Kt.3, Black is in no way at a disadvantage. The game may be declared even; and should White attempt to advance his Queen's pawns too vigorously Black would obtain the better game.—MEPHISTO.

TWO KNIGHTS' DEFENCE.

THE game having opened on the line of the Two Knights' Defence, or thus—

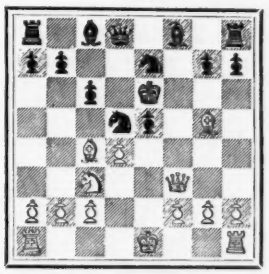
1. P. to Kt.4. / P. to K.4. 2. Kt. to K.B.3. / Kt. to Q.B.3. 3. B. to Q.B.4. / Kt. to K.B.3.

White has two ways of continuing the attack, and one line of play which we recommend to the learner as based on the sound principle of developing and strengthening his own position, rather than rushing at his opponents. This last-named line of play is simply 4. P. to Q.3., followed presently by Castles, B. to K.3. (or to K. Kt.5. if Black Castles early on King's side), Kt. to Q.B.3., and so forth; bringing all the pieces well into play before unduly pressing an attack. If Black plays a similar game, a steady but interesting and instructive game may be expected.

It seems, however, to be the generally-accepted view that first player has to attack and second player to defend; though theoretically, scarcely the slightest attacking power results from the first move, and what there is may be most readily lost. Accordingly, the usual way of continuing the Two Knights' game is either by 4. Kt. to K.Kt.5, or by 4. P. to Q.4. The latter is probably the sounder move. The former belongs to the class of premature attacks; though White wins a Pawn for the nonce if Black reply correctly. Black's best move in reply is 4. P. to Q.4, on which 5. P. takes P.

Black cannot now safely play 5. Kt. takes P., but we will follow out the interesting attack ensuing if he should play thus. There follows 6. Kt. takes K.B.P. / K. takes Kt. (Black must take the Knight, as it attacks both Queen and Rook), 7. Q. to K.B.3 / K. to K. 3. (the only play by which Black can save the Knight), 8. Kt. to Q.B.3. / Q.Kt. to K.2 9. P. to Q. 4. / P. to Q.B.3 (Black cannot take the Q.P. without losing the Knight by 10. Q. to K.4 (ch), 10. B. to K.Kt.5. The position is then:—

BLACK.



WHITE.

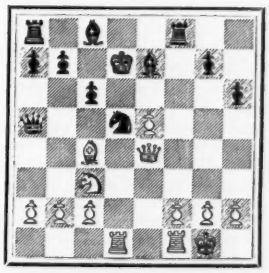
Position after White's tenth move.

It is obvious that White has now a very strong attack. If Black pushes his K. Rook's Pawn to attack the Bishop, White simply takes Knight with Bishop, and on Black's retaking (with Bishop, of course), White Castles on Queen's side with an overwhelming attack. If Black plays, instead, 10. P. takes Q.P., White Castles on Queen's side, with a crushing attack, whether Black take the Queen's Knight or not. If Black play 10. Q. to Q.R.4 White Castles on King's side, and after 11. P. to K.R.3 12. B. takes Kt. / B. takes B. there would probably follow—

13. Q.R. to Q.sq. / R. to K.B.sq. 14. Q. to K.4 / K. to Q.2 15. P. takes K.P.

and we have the position—

BLACK.



WHITE.

Position after White's 15th move.

Here White threatens to take the Knight either with Knight or Bishop, and if Pawn should retake to play R. takes P., winning Queen for Rook. If Queen move to Q.B.2, Q.Kt.3, or Q.Kt.5, or if King move to Q.B.2, White would take Knight with Bishop, and if Black then took Bishop with Pawn, White would win by Kt. takes P. (Note, that if Black King remains on Queen's file, or moves there from Q.B.2, he is exposed to a disclosed check by which his Queen's Rook would fall.) If Black plays away his King to K.'s square, White takes Knight with Knight, then, if Black retakes, White checks with Q. at K.Kt.'s 6th, winning the Rook (by B. takes P.), if Black interposes it, and the Queen (for Rook) if Black moves his K. on to Queen's file.

Returning to the position shown in the first figure, Black may play 10. P. to Q.Kt.4, on which White retreats his Bishop to Q.Kt.3, and if Black pushes his Q.Kt.'s P., White takes Kt. with Kt., and Castling on Queen's side obtains an overwhelming attack.

At his ninth move, Black might have played 9. P. to K.R.3, instead of 9. P. to Q.B.3; but 10. Castles, followed by 10. R. to K. sq., gives White an exceedingly strong attack.

In fact, on this line, Black's game seems hopelessly compromised; though we believe Zukertort is of opinion that this still remains to be proved, and that precisely as the Muzio attack, formerly thought irresistible, is now shown to be unsound*, so the sacrifice of the

* Even this, in turn, may be questioned; at any rate, Winawer played the Muzio against Zukertort in the Berlin Chess Tournament, and made a drawn game of it.

Knight in this form of the Two Knights' Game may, after all, be unsafe.

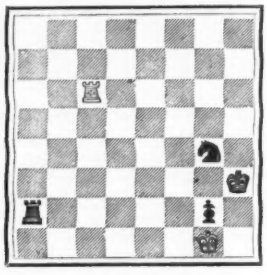
It appears to us that Black may perhaps find safety from 10. $\frac{\text{—}}{\text{P. to Q.K.4.,}}$ 11. $\frac{\text{B. to Q.Kt.3}}{\text{B. to Q.Kt.2}}$ (starting from position shown in first figure.)

Next week we shall consider what seems to be far a better course for Black than 4. P. takes Knight, namely 4. Kt. to Q.R.4.

We shall be glad to answer any questions suggested by the opening moves so far considered.

The following end game from Ponziani is of the same class as those given last week :—

No. 3. BLACK.



WHITE.

White to play, and draw.

Answers to Correspondents.

*** Communications which are to receive early attention should be addressed to the Editor of* KNOWLEDGE, *74 and 75, Great Queen-street, London, W.C.*

HINTS TO CORRESPONDENTS.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

W. H. ALLEN, J. W. BROOKES, A. L. J., PERPLEXED, and others. The illusion must be a strong one, as it remains, though, by a misprint, incorrectly described in Mr. Foster's paper.—CYMRO. Your mathematical query appears in mathematical section.—T. W. B. The work you mention is published by Messrs. Spon & Co., Charing-cross, who would inform you of the price. We do not know, and have no list of their works. We scarcely think "Anti-Guebre" would be satisfied by the account given of the sun in Huxley's Physiography—we mean that it would scarcely convince "Anti-Guebre" of the inaccuracy of his position. See Letter 5 in Number 2. The correct answer to the mountain-snow difficulty is not nearly so simple as many imagine. But of course "Anti-Guebre" is mistaken.—A. B., Manchester. Thanks for the promised papers on Photography. We have no doubt they will prove very interesting to our readers.—J. K. M. We quite agree with you, but technical terms which appear without explanation in articles meant for general reading are decidedly objectionable.—INQUIRER. (Your name is scarcely well chosen; there are so many inquirers.) He has made mistakes, but who that has been active in scientific research has not. He is, we think, *facile princeps* in his own field of labour, though we would not care to say so; comparisons (and the superlative is a form of comparison) are nearly always offensive.—CONICORD. Explain more fully your requirements. If you only want a general knowledge of the properties of the conic sections, Drew's "Geometrical Conic Sections," published by Macmillan, would serve your purpose admirably. But for thorough study of curves of the second degree, you would have to read more advanced treatises, which we will indicate, should you desire it.—GUEBRE. You will find the information you require in Lockyer's "Physics of the Sun" (Macmillan); but you will do well to read, as a more recent contribution to the subject, the excellent work on the sun, by Professor Young, Appleton & Co., New York. — EDW. CL. Your recommendations duly noticed, and will be attended to at once. Thanks for the addresses. — A. W., Edinburgh. All exceedingly welcome. — A. C. R. Could not send any copies of No. 1; none at the office could be spared. Twenty-five copies of No. 2 were sent.—FRANCIS MAZIN. Your suggestion has, you will find, been attended to. The pagination will now be right throughout, as title-page will replace advertisement leaf.—PYRAMIDALIST. We are not sure whether the interest in the Great Pyramid prophecies is so wide as you suggest; but you will find in a short paper by Mr. Jos. Baxendell, the well-known astronomer, much that will interest you.—DR. JULES MOREL. Many thanks for your kind expressions of sympathy with our plan. The work you have sent will doubtless afford much interesting information to our readers on the subject of food.—CYCLICAL. If you really want to study the Geometry of Cycloids at great length (but is it worth while?) we can only name "Proctor's Geometry of Cycloids," because we know of no other work going over the same ground. But, be warned in time, and take a lighter subject. If "wilful will to water, wilful must wet;" though, by the way, this is the last thing you have to fear, for the book is dreadfully dry. Cycloidal geometry is not of overwhelming importance in scientific study. We should say (but mind, we do not know for certain) that scarce a hundred copies of the work just named have been sold.—ANTI-HUMBUG. Your letter is quite unsuitable for publication in our columns. Its tone is absolutely contrary to that which we wish to maintain. If you contented yourself with the suggestion that those who differ from you are mistaken, we should still object to your letter; but your implied assertion that they *know* they are mistaken is intolerable. If you are so warm yourself against those whose opinions you oppose, consider, at any rate, that among them may be some as warm as yourself, whose feelings would be certainly hurt by your remarks and insinuations. In the words of one of the writers you mention, "Is life worth living?" if we are continually fighting?—TECHNIKER CHARLES KRAUS. We do not object to the trouble, on which alone you dwell, so much as to the expense; the three works you wish us to send you cost together 43s. Consider the effect of multiplied "orders" of that kind.—E. C. Articles of the kind sent are such as sub-editors make with the help of paste and scissors. We must not encroach on their sphere of labour, or, rather, on this portion of it.—W. L. Thanks for the drawings; albeit we by no means accept either the two-headed man or the alligator boy. The so-called man-dog and his son were real enough, though not correctly described even as to their appearance.—G. W. SPENCE. See First Hint to Correspondents. Delaunay's discussion about the Inferiority of Women to Men was a communication to a learned society, and has not, we believe, been published in separate form.—S. D. P. The razor-fish is not a fish, but a *Conchiferous Mollusc*, one of the *Solenaceans*, or *Dimyary Bivalves*. "I trust I make myself understood." Seriously, it is what is commonly, but incorrectly, called a shell-fish, the double shell somewhat resembling a razor.—STAR-GAZER. See answers to "Perplexed" and others.—W. T. LYNN. Thanks for the correction respecting Palitzsch and Halley's Comet; it shall appear next week, if possible. The mistake is definitely made in Herschell's "Familiar Lectures," p. 114, though, as you say, not in the "Outlines."—DR. H. DRAPER, New York. Many thanks.—G. W. D. Your letter is rather long, but shall appear, possibly abridged, as soon as possible.—G. A. Thanks for your interesting article on Buttercups; it shall appear next week.—J. M. A star atlas cannot include all desirable features; some must be sacrificed.—A. B. E. Thanks.—THOMAS WILTY, Hull. I scarcely think our readers would like to see any space devoted to the absurdities of the school you mention. It is hardly necessary for me to say that I repeatedly saw the sun south of the east and west horizon points when I was in Australasia. I have also repeatedly noticed that two and two make four.—R. M. See answer above to Mr. Mazin.—A. T. C. Yes. That point will be touched on in articles on Comets. A comet is not necessarily seen both when approaching and when receding, or even under either condition. The word unconformable, on p. 27, is not used in a geological sense, or, indeed, in a technical sense at all. It is put between inverted commas because borrowed (from Sir John Herschel); but every dictionary has it. The cribbage query is not suited to these columns; you will find that to make up numbers below nineteen the symmetrical arrangements which give large even numbers, as 6, 7, 7, 8, pairs royal, double pairs royal, are not so essential but that odd scores can be made. For scores above 18 you can only in one way get odd numbers, namely, by the "one for his nob," and this will not give 19, because the 18 which has to be made up to complete the score of 19 cannot be made up from a knave and four other cards; 20, 22, 24, 26, and 28 can. This, after all, shows only *how*, not why it is. The engraver, unfortunately, omitted one of the tails of the comet of 1744. Will consider your suggestion about covers.

E. L. R., I. C. H. W., BETA, W. A. R., J. A. W. O., J. A. W.=O., T. W. SHORE, W. D. R., CHARLES HAMILTON, △), G. C. W., G. G. D., J. McG. A, W. J. M., J. J. H. (B.A. Lond.), W. G. (BIRD'S-EYE VIEW), ARTHUR BLACK, H. S, &c. Letters received, and will appear or be answered in due course.

NOTE.—We have received many letters calling attention to the difficulty of obtaining copies of No. 1. The demand for copies was much larger than had been anticipated. The matter had to be entirely reset for a second edition, now publishing. Our publishers will be obliged if subscribers will call their attention to the fact, should they experience any difficulty in obtaining KNOWLEDGE at any of the railway book-stalls, or from their local newsagents. To ensure a supply of the back numbers, orders should be sent without delay.—ED.

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